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CEREBRAL DETERMINANTS OF AUDITORY LOCALIZATION

By EDWARD GIRDEN, Brooklyn College

With the increasing interest manifested in the analysis of the cerebral determinants of psychological processes, considerable progress has been made in the study of the visual mechanisms. Other systems, as for example the auditory, have been relatively neglected; with but few exceptions, the majority of the studies in this field have had as their objective the analysis of the peripheral (cochlear) functions. This report represents the results of the first of a series of investigations of the relationship between cerebral mechanisms and hearing.

Having completely transected the corpus callosum in a dog, Bikov and Speransky then conditioned the animal to salivate to the sound of a whistle of 1500 ~ presented from the dog's right side.[1] They were unable, however, subsequently to train the animal not to respond to the same stimulus sounded from its left side. That is, the dog salivated to this whistle regardless of its locus; it could not discriminate between left and right. Upon the basis of this experiment, Pavlov concluded that "a differentiation of the direction of a sound required a united activity of both hemispheres."[2]

When a sound is presented from the left or right side, some inequality is produced in the peripheral acoustical mechanism (cochleas) which is transmitted to the cortical system. Since the corpus callosum functions primarily in uniting the cortices of the two cerebral hemispheres, it would

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[1] K. M. Bikov and A. S. Speransky, Observations of dogs after section of the corpus callosum, quoted from I. P. Pavlov, *Conditioned Reflexes*, 1927, 150.

[2] I. P. Pavlov, *Conditioned Reflexes*, 1927, 150.

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seem logical to conclude, as did Pavlov, that the united action of the two hemispheres is required in the normal discrimination between left and

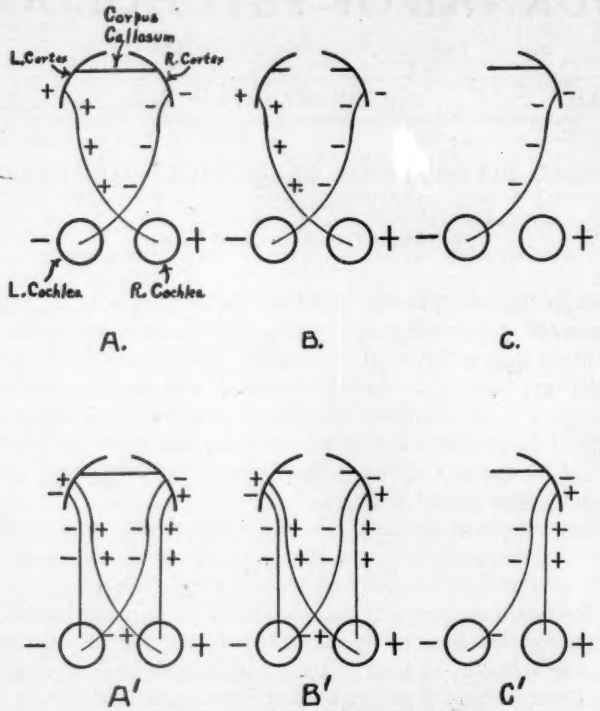

FIG. 1. DIAGRAMS OF THE AUDITORY SYSTEM

In the upper figures, A, B and C, hearing is represented as a completely crossed system, whereas in the lower diagrams, A′, B′ and C′, uncrossed as well as crossed components are indicated. The intervening synaptic levels between the peripheral (cochlear) and central (cortical) levels are omitted. The normal situation is posited in A and A′, transection of the corpus callosum in B and B′, and left hemi-decortication in C and C′. The sound is represented as coming from the animal's right side. The plus (+) and minus (−) signs represent the differential effect produced at the cochleas in terms of the physical qualities of the stimulus: phase, time, and intensity differences. Thus, the sound reaches the right ear first (as represented here), is more intense at that ear, etc., than at the left ear. See text for discussion.

right. This is consistent with the belief that hearing is crossed: all impulses from one cochlea (left or right) terminate in the opposite cortex (right or left). Since both ears are normally involved in the correct dis-

crimination of laterally produced sounds, transection of the corpus callosum would prevent the integration at the cortical level of the inequality produced at the cochleas. It would also seem to follow that a similar inability would occur with the extirpation of one cerebral cortex, as is produced by the destruction of one cochlea. This is indicated in Fig. 1, diagrams A, B, and C.

There is reason to believe, however, that the auditory system is not so simple. According to Kreidl,[3] hearing involves uncrossed as well as crossed neural components. Recent experimental evidence substantiates this view, indicating that in the dog[4] and the cat[5] the crossed and uncrossed components are approximately equal in acoustical value. Therefore, any acoustical inequality produced at the cochleas should have a representation on each cortex. From this logic, it would seem that the (left or right) hemidecorticate animal should be capable of localizing laterally produced sounds. Unless other clinical symptoms are produced by the operation, it should be possible to develop this discrimination in an animal in which the corpus callosum has been severed. This is illustrated in Fig. 1, diagrams A′, B′, and C′. As in the previous case, destruction of one cochlea would eliminate left-right discrimination.

A possible explanation of this contradiction may lie in the unreliability of the salivary (food motivation) conditioned response (CR) in studies involving the extirpation technique. Zeliony,[6] using this method, could not procure conditioned reflexes in a totally decorticate dog. Using the motor CR technique, Culler[7] produced evidence of both visual and auditory conditioning in a completely decorticate dog. These results were confirmed in the study of a second such animal[8] in which auditory, tactile, and thermal CR were developed as well as a discrimination between two auditory stimuli. In the light of these facts, the nature of the cerebral determinants of the L-R habit was studied by the motor conditioning method.

Method. A description of the technique of establishing motor conditioned re-

[3] A. Kreidl, Zur Frage der sekundären Hörbahnen, *Monatsch. f. Ohrenhk.* 48, 1914, 1-14.

[4] F. A. Mettler, G. Finch, E. Girden and E. Culler, Acoustic value of the several components of the auditory pathway, *Brain,* 57, 1934, 475-483.

[5] W. J. Brogden, E. Girden, F. A. Mettler and E. Culler, Acoustic value of the several components of the auditory system in cats, *Amer. J. Physiol.,* 116, 1936, 252-261.

[6] G. Zeliony, Effets de l'ablation des hémisphéres cérébraux, *Rev. de méd.,* 46, 1929, 191-214.

[7] E. A. Culler and F. A. Mettler, Conditioned behavior in a decorticate dog, *J. Comp. Psychol.,* 18, 1934, 291-303.

[8] E. Girden, F. A. Mettler, G. Finch and E. Culler, Conditioned responses in a decorticate dog to acoustic, thermal and tactile stimulation, *ibid.,* 21, 1936, 367-385.

sponses in dogs has been reported elsewhere,[9] and the modifications introduced in the study of the L-R habit have recently been described.[10] Usually the animal is first trained to flex forepaw to an auditory stimulus sounded from in front of the apparatus or from a position on the animal's right side. The L-R habit is successively developed for buzzers and then pure tones, so that the dog always flexes forepaw when the auditory stimulus is presented from its right side, but not when sounded from its left side. The sound-sources were placed a few inches from, and on line with, the left and right ears respectively. The buzzers were 6-volt bells with the hammers removed, and the receivers were Western Electric 705-A instruments. The pure tones, emitted through these receivers, were produced by means of a Western Electric 6-A audiometer having a frequency range from 128 to 9747 ~.

During critical tests of the L-R habit, the dog's head was held in a relatively fixed position by wooden neck-pieces.[11] Since the two buzzers were not equivalent, either in terms of tonal quality or intensity, they were switched (the left placed on the right side, and vice versa) not only on successive test-days, but at least once during each critical test. An analogous procedure was followed with the use of pure tones. Since the receivers were a matched pair, there was no need to switch them during the tests. On each critical test, however, the intensity was varied in chance order: sometimes the sound produced at the left side (LS) was louder, while on other trials the sound produced at the right side (RS) was louder. Successive presentations of the RS and LS stimuli, whether with buzzers or pure tones, were arranged in chance order.

Problems. The following experimental situations were studied. (1) Effect upon the L-R habit of complete transection of the corpus callosum (*Dog C*). (2) Development of the L-R habit after complete left hemidecortication (*Dog 2*). (3) Effect upon the L-R habit of extirpation of the temporal lobe and considerable adjacent parietal and occipital cortex of the right hemisphere (*Dog D*). (4) Effect upon the L-R habit of (a) bilateral temporal lobectomy on one operation (*Dog 4*); and (b) removal of the left and right temporal lobes in successive operations (*Dog 6*). After the critical tests were completed, the animals were dispatched and the brains fixed in formalin. Macroscopic examinations[12] were made to determine the extent of the lesions, which are represented here diagrammatically or by actual photographs. When necessary, microscopic (histological) examinations were initiated, and are now being completed.

Results. The results of the experiments on the different dogs may be summarized briefly as follows.

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[9] E. Culler, G. Finch, E. Girden and W. Brogden, Measurements of acuity by the conditioned response technique, *J. Gen. Psychol.*, 12, 1935, 223-227.

[10] E. Girden, Conditioning and problem solving behavior, this JOURNAL, 51, 1938.

[11] Cf. *Motor Conditioning in Dogs* (Film), C. H. Stoelting Co., Chicago.

[12] Dr. F. A. Mettler of the University of Georgia Medical School was kind enough to assist in the operation and make the macroscopic postmortem examination upon Dog C.

Dog C. The L-R habit for buzzers was established, with Dog C, June 24–July 12, 1935 (9 test-periods). After the operation on July 14, the animal showed severe behavioral disturbances. There would be periods of extreme excitement accompanied by spasmodic contractions of the muscles, particularly of the face. These attacks

FIG. 2. DIAGRAM OF THE BRAIN OF DOG C SHOWING EXTENT OF INJURY
The crossed area represents the cortex injured on the right hemisphere in the process of transectiong the corpus callosum.

could be produced by tactile or intense auditory stimuli. Erect posture was maintained only momentarily. Some improvement occurred by the following day, and testing was initiated on July 16. By the end of the third test, the animal's condition had become progressively worse, and he was then dispatched. The postmortem examination showed that considerable blood had clotted along the medial surface of the brain, the pressure of which probably contributed as a cause of the clinical picture. The corpus callosum had been completely transected. In performing this operation, the approach had been made by retraction of the right hemisphere which produced some damage to the parietal cortex, as shown in Fig. 2. This destruction involved none of the primary auditory cortex, the sylvian gyrus (areas 78 and 78b[12]).

TABLE I

EFFECT OF TRANSECTION OF THE CORPUS CALLOSUM OF DOG C UPON ITS LEFT-RIGHT DISCRIMINATION

The numerators of the fractions represent the number of CR; the denominators, the tota number of trials. Responses to LS are errors, and the total scores consist of the number of CR to RS plus the number of times no response was made to LS. The successive presentations of the stimuli on RS and LS were made in chance order and include at least one shift of the buzzers on every test. When pure tones were used (see Tables III–V) the intensity of the successive presentations was also varied in chance order. All of the pre-operative data of this animal have been reported in our earlier study.

Period	Date	RS	LS	Total Score
Pre-operative	7/ 9/35	8/8	2/18	24/26
	12	21/21	2/25	45/46
Post-operative	7/16/35	0/10	0/10	0/20
	17	7/18	0/6	7/24
	18	17/21	10/37	44/58

[12] Cf. J. W. Papez (*Comparative Neurology*, 1929, 1-518) for the procedure used here in identifying the cortical areas.

As indicated in Table I, the third and final post-operative test was the most satisfactory: 4 errors were made with RS, and 10 errors made with LS (since correct score to LS was no response). In other words, 81% correct responses were made to RS and 27% errors made to LS (July 18th). The difference, 54% (81% — 27%), is not attributable to chance. Statistically,[24] the ratio of this difference to its standard

FIG. 3. DIAGRAM OF THE BRAIN OF DOG D SHOWING EXTENT OF EXTIRPATED AND NON-FUNCTIONAL AREAS

Diagonals from left to right (\\\\) indicate the cortical areas which were extirpated, while the diagonals from right to left (////) indicate those areas which were rendered non-functional by being undercut.

error, ± .112, is more than 4.5, and therefore quite significant. This analysis is more than substantiated by the observations of the animal made in the test-situation. It might be argued that the CR and the L-R habit were actually being relearned. In the light of experience with many animals in this situation, this is but a possibility. No animal in the first stages of conditioning develops the L-R habit to the extent of making satisfactory scores with a chance order presentation of RS and LS stimuli, combined with an interchanging of the sound sources (see the discussion of Dog 4 below). There were also other criteria indicating the presence of the pre-operative organized L-R habit, such as appropriate incipient eye movements to the stimuli,[25] which only the well trained animal possesses. Some of the errors with LS are definitely accounted for in terms of periodic excitement in the test-situation, in part due to the animal's general heightened responsiveness to auditory and tactual stimulation. On those trials during which the animal was quiet, appropriate localization was made. If the animal was excited during the presentation of the LS stimulus (which could not always be anticipated) there was a tendency to flex forepaw.

Dog D. The L-R habit for buzzers was established, for Dog D, June 17–July 19, 1936 (25 test-periods). The animal was operated on July 20, and dispatched on July 23, 4 P.M. The postmortem examination showed that the following gyri of the right hemisphere had been extirpated: ectosylvian (68, 68b and 38b), sylvian (78 and 78b), polar (74) and insula (64). The following gyri were undercut and therefore non-functional: middle and posterior suprasylvanian gyri (48, 50 and 70), ectolateral (52), and part of the lateral (42). A macroscopic diagram of these le-

[24] Cf. H. L. Rietz, *Handbook of Mathematical Statistics*, 1924, 77.
[25] Cf. Girden, *op. cit.*

sions is presented in Fig. 3. The last pre-operative and the post-operative tests are summarized in Table II. A little uncertainty was noticed on the first post-operative test, and a little general excitement appeared on the third test. The second test, on July 22nd, was completely satisfactory: 100% with the RS buzzer and 0% response whenever the LS buzzer was sounded.

TABLE II

EFFECT OF RIGHT TEMPORAL LOBECTOMY OF DOG D UPON THE L-R HABIT
(For an explanation see Table I and the text.)

Period	Date	RS	LS	Total Score
Pre-operative	7/17/36	10/10	0/10	20/20 (100%)
	19	10/10	0/10	20/20 (100%)
Post-operative	7/21/36	20/22	5/22	37/44 (82%)
	22	10/10	0/10	20/20 (100%)
	23	12/13	2/15	25/28 (89%)

Dog 2. Dog 2 was operated at Urbana, Ill., Sept. 11, 1937, and recovered satisfactorily. (As can be seen in Fig. 4, the cortex of the left cerebral hemisphere had been extirpated almost completely, a slight bit of the medial-frontal cortex remaining intact.) Testing was initiated in Brooklyn, N.Y., Nov. 6, 1937, the animal first serving in some preliminary experiments not related to the present report. Training for this problem was begun on Nov. 24, and 90% level of left forepaw CR to buzzer sounded on dog's right side, was attained in two days (50 trials). Training with LS and RS stimuli was continued from Dec. 13–29 (14 test-periods). The first critical tests of the L-R habit were then tried, and the animal made close to perfect scores; that is, always responding to RS but inhibiting paw flexion to LS. Texts were continued until Jan. 6, 1938, at which time pure tones were introduced. A good score was made on this first test with three frequencies, but some difficulty occurred on the following day. After two subsequent tests with the buzzers, the pure tones were once again used. During these next few tests, the dog behaved in a

TABLE III

DEVELOPMENT OF L-R HABIT AFTER COMPLETE LEFT HEMI-DECORTICATION OF DOG 2
(For an explanation see Table I and the text.)

Date	RS stimuli				LS stimuli			
	256~	512~	1024~	2048~	256~	512~	1024~	0248~
2/22/38	8/10	4/4	4/4	4/4	0/4	0/6	0/3	0/4
24	6/7	6/6	3/3	3/3	0/3	0/4	0/3	0/4
25	5/5	3/3	3/3	5/5	0/6	0/2	0/4	0/5
26	5/5	5/5	7/7	5/5	0/4	0/4	0/5	0/5

fashion which can only be best described as neurotic. Whether this was due to the poor health of the animal at this time, or the use of test frequencies above 2048 ~, is not clear. For example, the dog would reach towards and attempt to *eat* (attack?) the RS receiver. During some trials with this stimulus, spasmodic flexions of hind limbs or twitchings of the head appeared; again, these responses could be modified and at times completely eliminated by the immediate cessation of RS and the introduction of the LS tone. The LS receiver was *never attacked*. In order to complete the tests with the lower pitches, the frequencies above 2048 ~ were then eliminated.

After a few days the animal improved considerably, physically and in the elimination of neurotic symptoms; concomitantly, the tests with the lower frequencies became more reliable. The animal had no difficulty in correctly localizing the four test-

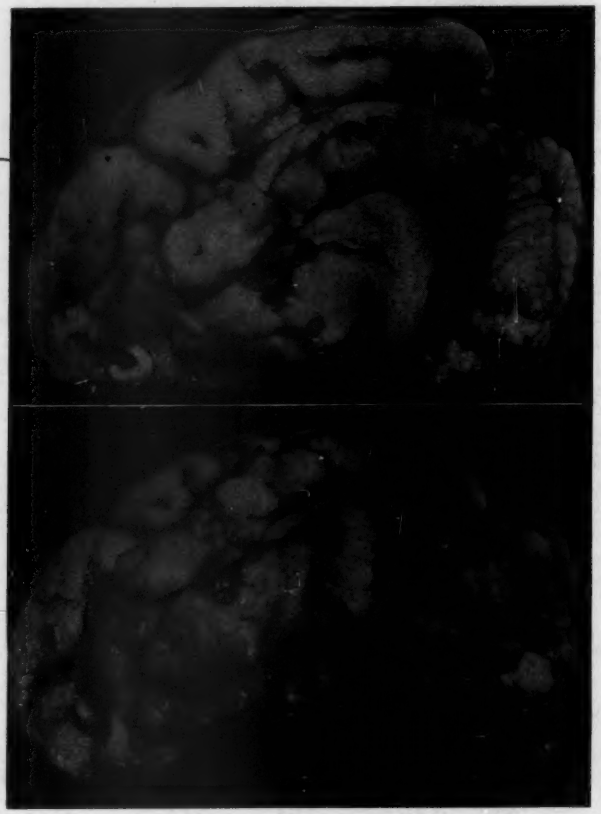

FIG. 4. LATERAL VIEWS OF THE LEFT HEMISPHERE OF DOG 2
These photographs show that all the cortex, except a slight piece of the frontal-medial surface, was ablated.

frequencies used: 256, 512, 1024 and 2048 ~ respectively. The scores of the last four tests are given in Table III. These scores, as in the other tables, represent the totals for each frequency, and do not indicate the order of presentation or the variations of intensity used. For example, on Jan. 25 (Table III), the test was initiated with a pitch of 2048 ~, and proceeded as follows: (1) RS (80 db.) 2 trials; (2)

LS (85 db.) 1 trial; (75 db.) 2 trials; (3) RS (75 db.) 3 trials; (4) LS (80 db.) 2 trials. The next frequency tried was 1024 ~ (LS), and the test proceeded in a similarly treated chance-order fashion. The animal was dispatched Jan. 28, 1938.

Dog 4. Training of Dog 4 was started Nov. 6, 1937. Tests with RS and LS buzzers were started Nov. 19, and pure tones were introduced Dec. 25. The CR was

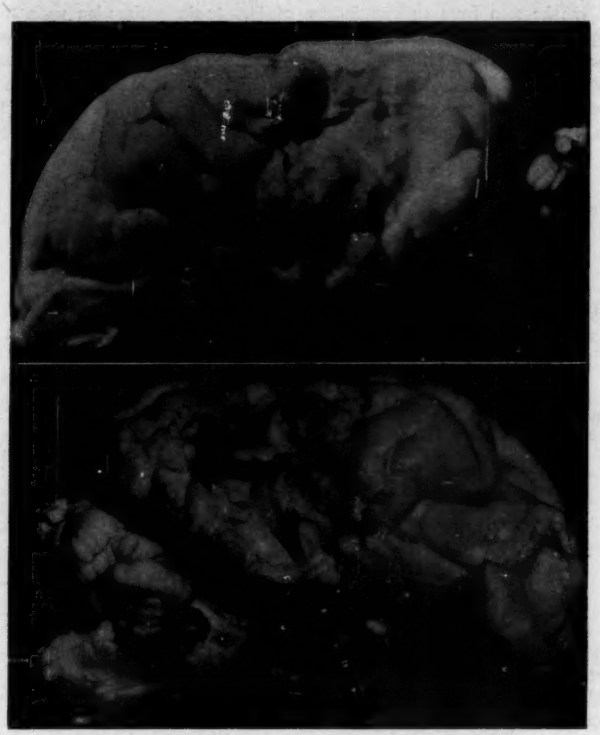

FIG. 5. VIEWS OF THE CORTEX OF DOG 4
These photographs show the areas extirpated from both temporal lobes in a single operation.

established very quickly. Due to a tendency to overactivity, which resulted in erratic performances, the training with the higher frequencies progressed slowly. Satisfactory tests were finally procured in February 1938. The animal was operated upon Feb. 19th, and the auditory areas of both the left and right hemispheres were removed in a single stage. The animal was depressed for several days afterwards, requiring artificial feeding. By Feb. 22, the dog had recovered sufficiently enough

to eliminate the force-feeding, and was able to walk around. Cortical blindness and deafness were present. Pupillary response to strong light was weak and sluggish. Food was located by muzzle-sensibility, and objects were not avoided. The normal behavior to usual sounds (feeding of other animals by the attendant, approach of the experimenter to its cage), was absent. Testing was initiated Feb. 25, and the animal was dispatched March 24, 1938.

As can be seen from an examination of Fig. 5, the extirpation upon the left hemisphere was limited to the cortex, but on the right hemisphere the ablation was somewhat deeper, the exact extent of which cannot be determined until the histological examination is completed. The changes produced in localization habit cannot be attributed to relatively deeper lesions produced in right hemisphere. These lesions are far from being as deep or as extensive as those produced in Dog 2 (cf. Fig. 4).

TABLE IV

EFFECT OF SIMULTANEOUS EXTIRPATION OF LEFT AND RIGHT TEMPORAL LOBES OF DOG 4 UPON THE L-R HABIT

(For an explanation see Table I and the text.)

Period	Date	Side	Stimul								
			Buzzer	128~	256~	512~	1024~	2048~	4096~	8192~	9747~
Pre-operative	2/14/38	RS	—	2/2	1/1	2/2	2/2	4/4	1/1	3/3	3/3
	17		—	3/3	2/2	3/3	2/2	5/5	3/3	2/2	3/3
	2/14/38	LS	—	0/2	0/2	0/5	0/3	0/5	2/5	0/3	2/11
	17		—	0/3	0/2	0/2	0/2	1/10	0/6	0/3	0/3
Post-operative	2/25/38	RS	4/10	—	0/2	0/3	0/3	0/3	0/3	0/1	—
	26		11/18	—	—	0/3	0/5	0/2	3/4	3/6	—
	3/ 1/38		1/1	—	0/1	0/3	1/7	6/9	4/9	3/6	—
	2/25/38	LS									
	26		5/6	—	0/3	—	—	—	1/1	—	—
	3/ 1/38		—	—	—	—	—	3/4	2/3	2/8	—

As indicated in Table IV, the CR as well as the L-R habit was disrupted by the operation. Subsequent tests show incipient signs of the reappearance of a conditioned response, more rapidly with the buzzer than the pure tones. At the conclusion of the test on Mar. 24 (the last of four tests given after those shown in Table IV) the CR was beginning to appear regularly. The L-R habit was still completely absent, the dog responding as often to the LS as to the RS stimulus. It seems clear that this is evidence for relearning of the CR. If the latter had been simply depressed because of post-operative shock, its reappearance would have been more sudden and rapid (cf. Dogs C and D in contrast to these results).

Dog 6. Left forepaw CR to RS buzzer was started with Dog 6 on Nov. 15, 1937. Regular tests with RS and LS buzzers were begun on Dec. 23, and the animal readily developed the L-R habit. The extirpation upon the left hemisphere was made on Feb. 9, 1938. The auditory area of the right hemisphere was extirpated in two

stages: Mar. 3, and Mar. 17 respectively. The amount of cortex ablated from the right hemisphere was less than the total removed in the single operation performed upon the left hemisphere (cf. Fig. 6).

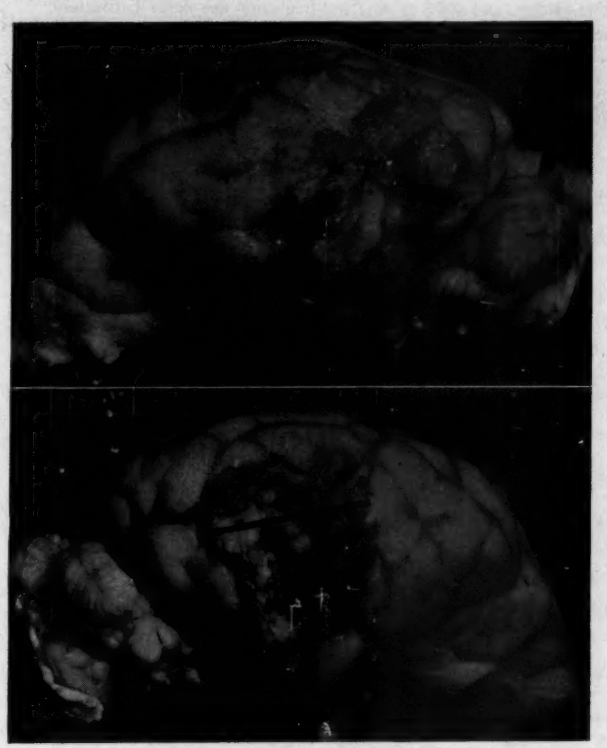

FIG. 6. VIEWS OF THE CORTEX OF DOG 6

The cortex of the left hemisphere was extirpated in a single operation. The lesions produced on the right hemisphere were then made in two subsequent operations. The cortex above the black line extirpated first, followed by the removal of the areas shown below the black line.

The effect of the successive extirpations are summarized in Table V. During the pre-operative training all octave frequencies were used at one time or another. Some difficulty was encountered with 2048 and 4896 ~. This resulted in part from an attempt to set up tonal discriminations so that the response was inhibited to 2048 ~ whether it was the RS or LS, and at the same time make 4096 ~ positive so that it was responded to, whether as LS or RS. Immediate results indicated that

this procedure would require extensive training, time for which was not available then. Since this procedure tended to disrupt the L-R habit for the other frequencies, it was halted. As a consequence, the L-R habit for 2048 ~ was not on par with the other pitches, and 4096 ~ as a test-frequency was never satisfactory.

The removal of the left auditory cortex (first operation) had the same effect as

TABLE V

EFFECT OF SUCCESSIVE EXTIRPATION OF LEFT AND RIGHT TEMPORAL LOBES OF DOG 6 UPON THE L-R HABIT

(For an explanation see Table I and the text.)

Date	Side stimulated	Stimuli						
		128~	256~	512~	1024~	2048~	8192~	9747~
2/6/38	RS	1/1	3/3	3/3	4/4	5/5	3/3	—
	LS	0/2	0/2	0/3	0/5	0/5	0/4	—
2/7	RS	—	5/5	4/4	1/1	3/3	3/3	—
	LS	—	0/7	0/2	0/3	0/5	0/5	—
2/8	RS	6/6	3/3	3/3	4/4	4/4	4/4	—
	LS	0/4	0/4	0/3	0/5	0/4	0/5	—
2/9	First entry: left side							
2/10	RS	3/3	5/5	7/7	8/12	9/11	5/5	—
	LS	0/3	0/5	0/5	0/6	5/14	0/8	—
2/11	RS	3/3	5/5	5/5	4/4	10/12	4/4	4/4
	LS	0/5	0/6	0/4	0/4	0/6	0/5	0/3
2/12	RS	—	5/5	5/5	3/3	5/5	3/3	3/3
	LS	—	0/4	0/6	1/7	0/6	0/5	0/4
2/14	RS	2/2	2/2	2/2	1/1	2/2	3/3	2/2
	LS	0/1	0/3	0/3	0/3	0/3	0/2	0/3
3/4	RS	1/1	2/2	1/1	3/3	3/3	2/2	2/2
	LS	0/1	0/1	0/3	0/2	0/4	0/3	0/4
3/4	First entry: right side							
3/5	RS	3/4	6/8	2/2	3/3	4/4	3/3	2/2
	LS	0/2	0/2	0/3	0/2	0/3	0/4	0/2
3/14	RS	1/1	2/2	2/2	1/1	3/3	4/5	2/2
	LS	0/3	0/3	0/3	0/3	0/3	0/5	0/3
3/17	Second entry: right side							
3/18	RS	0/3	0/3	0/4	0/4	0/6	0/4	0/3
	LS	—	—	—	—	—	—	—
3/19	RS	5/6	2/6	6/10	8/13	8/15	4/4	2/2
	LS	7/10	2/2	5/10	5/6	2/2	2/2	2/2
3/21	RS	0/2	6/6	3/3	2/2	1/1	2/2	2/4
	LS	—	3/3	6/7	2/2	2/2	1/1	2/2

the removal of the right side in Dog D: the L-R habit persisted undisturbed. The restricted lesions on the right side (second operation) again produced no significant changes. As shown in Table IV, somewhat less than perfect scores were made with 128 and 256 ~ on Mar. 5, but the animal was entirely satisfactory on the other test-frequencies. On the second test (Mar. 14) the score was uniformly perfect. It is safe to conclude, therefore, that this area of the cortex on the right side had no functional significance for the L-R habit. The second entry on the right side (third

operation) not only completely eliminated the L-R habit but temporarily depressed the CR as well for all test-frequencies. In spite of 11 shock-reënforced trials out of a total of 27 trials on the first post-operative test (Mar. 18th), there was no evidence of the conditioned response. The test was made but 23 hr. after the initiation of the third operation, and it is likely that this test-result was a consequence of operative shock. On the following day, there was almost complete recovery of the CR. There was, however, no sign of the L-R habit, then or later, the dog continuing to respond indiscriminately to both RS and LS stimuli.

Discussion. In a study of perceptual processes in animals, it is evident that the validity of the investigation depends in part upon the technique that is used. On occasion, (negative) results procured by one method have been supplanted by (positive) data gathered by means of an entirely different technique. As an example, experiments in which the electrical method was used to determine the function of the round window in hearing,[16] have been negated by studies in which the motor technique was employed.[17] Ruckmick's suggestion, that results procured by an electrical approach should be checked by a concomitant behavioral study, is pertinent.[18] Furthermore, negative results procured with one behavioral approach need not be conclusive. Definite proof of pattern vision in the rat was not established until the introduction of Lashley's "jumping" apparatus. The contradictory results procured in the study of conditioned reflexes in totally decorticate dogs by the salivary and motor methods have already been discussed.

A similar discrepancy occurs between Bikov's negative findings and the present data. When tested by the motor CR technique, the localization of sounds persists following transection of the corpus callosum. One can only conclude that the salivary technique is not as reliable as the motor conditioning method in extirpation studies. The L-R discrimination likewise is shown to persist following extensive lesions in the cortex of either the left or right cerebral hemisphere. These findings, as well as the development of the L-R habit in the completely hemi-decorticate animal, were shown to be more consistent with the present views concerning the organization of the acoustic pathways.

The normal functioning of the L-R habit requires the establishment of some differential acoustic effect at the cochleas: time differences, intensity differences, and phase differences. The cortical mechanism involved in this

[16] W. Hughson and S. J. Crowe, Function of the round window: An experimental study, *J. Amer. Med. Assn.*, 96, 1931, 2027-2028.

[17] E. Culler, G. Finch and E. Girden, Function of the round window in hearing, *Amer. J. Physiol.*, 111, 1935, 416-425.

[18] C. A. Ruckmick, A critical review of the field of audition, *Psychol. Bull.*, 33, 1936, 407-431.

discrimination is not indiscriminately spread through the cortex as a whole, but is limited to a specific area, probably within the limits of the sylvian gyrus. The present data would seem to indicate that not only is there a bilateral auditory representation (which conclusion has already been made by previous investigators), but that there is also a *bilateral cortical representation for both cochleas;* impulses from both cochlea terminate on each of the cerebral cortices.

The following interesting question is immediately suggested: Do particular parts of the critical area function in the perception of specific auditory frequencies, or is there equipotentiality of function for the L-R habit? Pennington's interesting studies[19] have been directed to the solution of this problem. The present data are in agreement with his findings in the white rat, that there is a critical cortical area functioning in this discrimination. Likewise, the L-R habit is interfered with, only if there is a bilateral cortical extirpation. He reports, however, that the auditory localization habit depends upon the amount of functional cortex within the critical area: the amount of interference is in proportion to the amount of cortex extirpated.

In his experiments, Pennington used buzzers as stimuli, and the validity of his interpretation is questionable because of this fact. Let us suppose for the moment that specific parts of the critical area function in the perception of specific frequencies. If noises are used as the stimuli, the extirpation of particular parts of this area would eliminate the perception of particular frequencies. Since auditory localization is more difficult for pure tones than for complex buzzer-noises, the more cortex removed the more frequencies that cannot be perceived; hence the L-R habit becomes progressively disrupted. A similar effect could be produced by extirpating progressively more and more frequencies from the acoustic stimulus. That is, by the use of proper sound-analyzers, one could present a number of auditory stimuli varying in complexity in wave-form from noises to single frequencies. One would under these circumstances, get a varying amount of accuracy in the L-R discrimination habit.

A conclusive test of this question can be made only if pure tones are used as stimuli. If the L-R habit is disrupted equally for all frequencies regardless the size or locus of the lesions (within the critical area), one can safely conclude in favor of a theory of mass action. If on the other hand, specific lesions effect the loss of the L-R habit for particular tones, one can conclude that particular parts of the cortex function in the per-

[19] L. A. Pennington, The function of the brain in auditory localisation: IV. Method of training and control experiments, *J. Comp. Psychol.*, 25, 1938, 195-211.

ception of particular frequencies, *in so far as the L-R habit is concerned.* While the present data include tests with pure tones, the results do not bear directly upon this question. One might argue that since the animal responds to the particular frequencies, such a mechanism exists. An unequivocal test of the question is, therefore, being made, by studying the effects of localized lesions upon the L-R habit for pure tones. Until such results are procured, further speculation at this time would be fruitless.

Conclusions. In contradiction to the evidence procured by the salivary CR method, it is found that the L-R auditory localization habit persists following complete transection of the corpus callosum. This discrimination can also be developed in the completely hemi-decorticate animal. The relationship of these findings to auditory theory and the significance of the difference between the two methods is discussed.

The present data, corroborate previous findings reported in the white rat; the L-R habit is a function of a restricted area of the cortex within the temporal lobe. The two areas of the left and right hemispheres can act in unison or separately, as the case may require: the L-R habit persists following the removal of the auditory cortex of either the left or right cerebral hemispheres. The application of the present technique in the further exploration of cortical functions in hearing is indicated.

THE PROBLEM OF SET

By G. L. FREEMAN, Northwestern University

The experimental psychologist exercises only a relative control over the phenomena which he investigates. He can set up and manipulate certain of the determining conditions, hoping that those conditions of which he knows little or nothing will operate in a constant direction or else will cancel each other's effects over a long series of observations. If the experimentalist is lucky, he can state a relationship between the phenomenon under investigation and the conditions which he has varied. If he is unlucky, he must assume the operation of some other conditions over which he exercised little or no control. Probably the most hackneyed of these alibi conditions is 'set.' For example, it is very convenient to explain a capricious relationship between the span of apprehension and stimulus intensity as due to shifts in the subject's attitude or set. This may be a shrewd guess in the right direction, but when dragged in as an afterthought, the suggestion is not very illuminating. One cannot make such an ill-defined concept the scapegoat for his failure at experimental control by the mere saying of words. The truth is that most researchers give little attention to the problem of set, having only the vaguest notion of the conditions referred to the term and being very naïve as to their experimental check and control. In the zeal to connect 'stimulus' with 'response,' this important determinant of performance is frequently ignored.

By common agreement the term 'set' refers to those vaguely defined organic states, conditions or processes which are antecedent to some overt reaction-pattern and which run along with and so exercise a selective and determining influence upon that reaction. Introduced into psychology to explain aberrations in behavior not easily associated with the external stimulus, it has had a confused history. This is indicated by the variety of terms which have been used in similar contexts—*Einstellung, Aufgabe, Bewusstseinslage, determining tendency, predisposition, posture, readiness,* and *anticipatory reaction.*

The importance of set was first recognized in experiments on reaction-time, where it was found that preliminary attunement aroused by the ready-signal increased the speed of subsequent response. This was followed by the distinction between the 'sensory' and 'motor' reaction-sets and the study

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of antecedent organic adjustments in the judgment of lifted weights. *Aufgabe* and the importance of the *foreperiod* appeared with experiments upon the action-consciousness; Külpe emphasized the importance of the 'task set' in attentive preparation, and in their analyses both Watt and Ach noted wide differences in formulated tasks. Messer went further to include non-conscious predispositions under the term. While Müller regarded the determining tendency of Ach as 'voluntaristic' and 'mysterious,' he substituted something very much like it in his 'goal idea,' and showed that in the learning of nonsense syllables one item came to carry a 'reference' to the next in the series. Introspective studies of thinking gave brief glimpses of the progress of determining tendencies; Mayer and Orth applied the term *Bewusstseinslage* to the 'flash' reports which informed the thinker of his position on the matter under consideration. In the argument which turned upon the question of their attributive status, the functional significance of these conscious attitudes and determining tendencies was all but lost. While their importance as governors of performance appears again in Köhler's 'insight,' Lewin's 'tensions' and Tolman's 'means-end-readiness,' in such contexts the determinations partake too much of the nature of psychical agents and powers to be generally accepted.

Work on the concomitants of response in directed activity has secured the interest of students of behavior in the problem of set. Dodge's 'anticipatory reaction,' Woodworth's 'prepared act,' and Dashiell's 'postural response' are attempts to conceive these important organic conditions in muscular terms. Experiments on the preparatory muscular adjustments of various acts give body to the behavioral point of view without adding much by way of clarification. Regarded systematically, all that is exhibited is the tendency for certain antecedents of activity to express themselves in terms of a muscular *'Einstellung.'* Bills' 'quantitative set,' Young's 'organic' dispositions, and the various work preparations found in the researches by Jersild, Poffenberger and Robinson are not to be reduced so simply. While some assume muscular concomitants not only for residues of preceding acts but also in more remote preparations of a task to be performed, psychologists are generally inclined to regard the problem of set as fundamentally neural; Titchener's 'brain habit,' Von Kries' 'connective disposition,' and Thorndike's 'readiness to respond' are typical examples. But the problem is not to be shifted thus easily to the backs of the physiologists (who are surprisingly ignorant of its existence): it compels the further attention of psychologists themselves. Useful discussions are rare, but where these appear, considerable has been done in bringing a descriptive order to the field and in pertinent theorizing.

Discussing sets as 'antecedents of function,' Bentley mentions two ways in which they operate to limit and define the activities of the organism.[1] The *predisposition* is "a general readiness, such as comes from long study or employment, or from devotion to an art. We are always 'tuned' for some functional performance, although the tuning often passes unnoticed for want of an occasion to touch it off." The *specified task* exists "where an anticipative interest is present. . . . It is more than a mere tendency to function. The organism . . . formulates an obligation or at the least an interest. When the formulation lapses, as it does regularly under repetition, there commonly remains the . . . predisposition." Bentley gives a helpful account of the various conditions and forms of preparation which the psychologist has been able to describe (including task, formal, occasional and self instruction, fore-period and determination) and adds "what we especially need is a physiology of the mechanisms by which a functional inclination in the central nervous system is made up and sustained while the action progresses on its way to movement. In the brain there is doubtless a specific harboring of energy which is integrated on the one hand with the central processes underlying apprehension and, on the other, with the orderly and systematic use of the mechanisms underlying movement. . . . Since we cannot accept a special governing center of automatic and arbitrary powers or a more imperious will, we must look for highly patterned processes within the brain which sustain and guide the nascent determination toward its ultimate outcome."

In Dashiell's treatment,[2] sets are classified as postural responses. After distinguishing between phasic and static forms of reaction, a strong case is made for bodily postures and sets as static responses which operate to support, reënforce, inhibit or otherwise affect the more short-lived (phasic) movements. Attention is a "form of posturing" which favors overt response to some particular stimulus. Many emotional reactions include postural alterations which 'set' the organism for ensuing response. The effect of previous sensory reactions is to establish a 'prejudicial set.' Here and elsewhere are suggestions of a dual response-mechanism, phasic and static, each part contributing to the behavior flux and each influenced by the other. But while recognizing that phasic and static elements exist in all response, Dashiell is not prepared to set up a hard and fast dichotomy and questions where in the total pattern of muscular adjustments one type of action begins and the other leaves off. It is unfortunate that Dashiell does not develop this theory more fully, especially since its difficulties become more apparent when considered in detail. Implicit in his writings is the assumption that the effect and influence of set is registered and produced in the efferent part of the reaction-arc. We are told that "behavior manifests some narrowing down of motor output to certain particular channels." Postural differentiation apparently holds for certain forms of prepared action—as in sensory and motor reaction sets; but can the 'set to add' and the 'set to subtract' be so differentiated? On the contrary, it would seem that both sets utilize the same motor channels, and that their differentiation must be central.

While Woodworth agrees with Dashiell in seeking a response interpretation of set, his analysis leads him in another direction.[3] Instead of relating sets to a particu-

[1] M. Bentley, *The Field of Psychology*, 1927, 388; *The New Field of Psychology*, 1934, 229.

[2] J. F. Dashiell, *Fundamentals of Objective Psychology*, 1928, 275; *Fundamentals of General Psychology*, 1937, 339.

[3] R. S. Woodworth, *Psychology*, 1934, 327; Situation-and-goal set, this JOURNAL, 50, 1938, 130.

lar class of responses, Woodworth, by implication, makes them a tentative activation of the response which they anticipate. We are told that "the preparatory set for an act consists in going as far as possible towards the execution of the act, without actually releasing it. To adapt our S-O-R formula to prepared reactions we have to remember that the preliminary signal, P, arouses a readiness to make a certain response and then comes the stimulus, S, and releases that response. Preparation consists in making a start toward R; S finds . . . activity R already in progress but suspended and gives O the necessary nudge to release the complete R." We shall not pause with the equivocation involved in "suspending" activities which are "in progress," but turn at once to the core of Woodworth's argument. This seems to be that the awaited stimulus acts as a trigger releasing to full potency a pattern already activated. By implication the stimulus does not change or contribute anything toward the character of the reacion. It merely increases the strength of this pattern so that what was implicit and covert becomes explicit and overt. The preparatory signal is only a part of the larger stimulus-pattern which conditions the response. Accordingly the response must exist in a tentative state until the rest of the stimulus-pattern appears, only then by the summation of energies is the threshold of overt reaction reached. This notion is especially attractive in explaining sets for reactions which are distinctly motor. If similar analysis could be successfully advanced for all types of performance, there would be little to condemn this logical extension of Woodworth's theory. But when similar muscular patterns are found to support the higher psychological functions, it is hard to accept the principle that set is a tentative manifestation of an about-to-be-overt motor response. The notion of a specific sensorimotor arc sensitized during the pre-stimulus period by slight contractions in the muscles which form the terminus of that circuit is attractive in its simplicity. But the neuromuscular mechanism is something more than a mere congeries of isolated reaction-arcs, and we should approach with great soberness any conception of set which leads implicitly in this direction.

Hoisington's analysis of set adds measurably to the descriptive side but reaches no concrete theoretical formulation.[4] He points out that 'sets' are a type of reflex integration functionally distinct from the "specific reactions" made to sensory stimuli. The responses which are the determining tendency "are general in character, muscular tensions rather than overt movements. They start with and grow through the exercise of more specific task or problem sets. They arise out of the performance of the several (specific) tasks which they come to dominate." The relational effects of specific activities are apparently registered in certain integrating centers, for we are told that "it is not unusual for the product of integration to come to dominate further functional interaction . . . mathematical determination arises with the doing of mathematics problems, but once developed, it determines overt ability in, and methods of attacking, this kind of problem." Hoisington escapes the natural tendency to assign specific neuromuscular tensions to all his determinations, implying that channalization may be central. Determining tendencies or sets may have varying degrees of specificity with regards to the activities which they anticipate and control. There may thus be 'sets' within sets, a possibility which prompts the question "was there one original task-set out of which all other sets and determinations arose, or do they all finally reconverge in some master determination; or does some

[4] L. B. Hoisington, *Psychology: An Elementary Text*, 1935, 133.

one tendency spread over the total organization so that it becomes characteristic of all (more specific) determinations?"

It can be seen that in describing the phenomena of set each account calls attention only to certain aspects of the problem; Bentley and Hoisington distinguish between 'sets' of various kinds; Dashiell mentions the difference between 'sets' and the overt acts which they sustain; Woodworth points out their similarity. All this is very confusing to the critical student. Instead of working hypotheses capable of being tested, he is faced with vague descriptive generalizations or theories quite inaccessible to experimental attack and criticism. This state of affairs is largely responsible for the relative neglect of the phenomena of 'set' both in laboratory procedures and in the systematic setting of related problems.

The present report attempts to construct a working hypothesis of 'set' in terms of postural responses and central nervous function. This hypothesis is developed in a series of six propositions, on each of which information is either already available or obtainable by direct experimentation. We consider the six propositions in order.

(1) Reaction to stimulation involves both phasic and tonic response-segments. Study of the neuromuscular mechanism shows that it is capable of reacting in two ways, phasically and tonically. This fact is first met in the distinction between tonic and phasic muscular contraction.[5] Its importance in integrated behavior is also appreciated. Beginning with Sherrington's 'reflex figure' and continuing to Walshe's 'associated reflexes,' physiologists have realized that response includes not only activity directed specifically toward the source of stimulation, but also widespread supporting postural adjustments.[6] From another direction, evidence accumulates that muscular tension supplies a major share of cortical excitation, and is responsible for lowered sensory thresholds during waking activity.[7]

[5] For Sherrington, the same motor connections are involved in both cases, the muscle contracting tonically in response to the stretch of its proprioceptors and phasically in response to exteroceptor stimulation (C. S. Sherrington, Postural activity of muscle and nerve, *Brain*, 38, 1915, 191-234). Hunter assumes that different fibers, red and white, are active in each case (J. Hunter, The postural influence of the sympathetic nervous system, *ibid.*, 1924, 47, 261-274); while Hunt distinguishes the part of the muscle-tissue stimulated, with the sarcostyles giving phasic and the sarcolemma giving tonic movements (J. R. Hunt, The dual nature of the efferent nervous system, *Arch. Neur. & Psychiat*, 10, 1923, 37-82). Hunt also suggests that the two types of activity have differing control centers in the pyramidal and extrapyramidal systems.

[6] Cf. C. S. Sherrington, *Integrative Action of the Nervous System*, 1906, 1-200; L. M. Walshe, *Med. Sci. Abst. Rev.*, 7, 1923, 109-118.

[7] Cf. Edmund Jacobson, Further experiments on the inhibition of sensations, this JOURNAL, 23, 1912, 345-369; and L. H. Cohen and S. Lindley, The relationship

The phasic segment of response is of short duration and usually represents a temporary adjustment to some momentary and fleeting stimulus. The tonic response-segment represents a more enduring type of adjustment, acting to maintain a certain continuity in the organism's conduct. Phasic acts of voluntary or reflex nature occur only when the muscles are in a state of slight tension. Special types of response cannot be made unless the pattern of tonic contraction expresses itself in a given posture. Except for such postural adjustments, the inception of a voluntary act would find the part to be moved in an unsuitable position, as is the case in locomotor ataxia. Phasic intraneural sequences seem equally dependent for their maintenance upon a steady backflow of afferent impulses from the tonic processes Just as the taximeter translates distance into cents, so the final product of postural change is measured and new phasic orientation related thereto. Regardless of the particular type of neuromuscular action under investigation, the mutual contributions of the tonic or postural segment and the phasic or kinetic segment are always apparent.

(2) Tonic response-segments may come to anticipate the associated phasic response in the process of repeated stimulation and 'conditioning.' While the concurrence of tonic and phasic response-segments is generally accepted, the principle that the tonic aspects of the pattern may precede as well as outlive the phasic aspects requires demonstration. Unaccustomed to thinking of the total response-pattern as involving two elements, layers, or divisions of activity, experimenters have given their major attention to the phasic aspects of response. These are the most obvious features of neuromuscular action, but not necessarily the most important. It is probable that explanation of response variability in the face of apparently constant stimulus-conditions rests upon the fact that these aberrations are not strictly phasic in character but are built on a *postural substrate*—a neuromuscular background of tonic origin which operates to facilitate, inhibit, and otherwise to modify phasic reactions. Very little is known of the time-relations between the tonic and phasic processes, although there is some evidence that the postural response outlasts its phasic concomitant.[8] If the tonic

of muscle tonus changes to vibratory sensibility, *Psychol. Monog.*, 47, 1936 (no. 212), 83-93. The relation of tonic contraction to consciousness is discussed by the writer (G. L. Freeman, Mental activity and the muscular processes, *Psychol. Rev.*, 38, 1931, 428-449). For experimental studies from this laboratory cf. R. M. Simpson, The effect of experimentally induced muscular tension upon sensory thresholds, (in press).

[8] Without the postural substrate, man's behavior would undoubtedly resemble that of a complicated jumping jack. The patterning of this substrate exhibits considerable inertia to change, forming a frame of reference which anticipates the next response and thereby renders consistency to the more fleeting aspects of reaction. (Cf. Dashiell, *op. cit.*, 317-321.)

segment continues beyond a given phasic response-segment, it would thereby precede the next phasic movement. In many performances, such as playing tennis, the postural adjustment (stance) may remain relatively unchanged during several similar phasic acts (returning the ball). While it is difficult to say just where in the total pattern of muscular adjustments one segment begins and the other ends, it can be seen that those segments which are antecedent to a given phasic reaction are theoretically able to exercise a directive influence over it. To assert that this principle holds generally, however, raises the question of how one part of a reaction-pattern comes to anticipate and govern the remainder. We know very little of the actual mechanics involved here, but we can set up an hypothesis capable of experimental check.

Let us suppose that a stimulus configuration (S_1S_2) evokes a response pattern (R_1r_2) which has both tonic and phasic elements. In the resulting complex it is reasonable to assume that tonic and phasic factors may be captured by different parts of the stimulus-configuration. The phasic element will be associated with the most dominant part of the stimulus-configuration and its character will be determined by connections of low innate 'resistance.' This is the 'unconditioned response' of Pavlovian terminology. What, however, of the tonic response-elements so generally ignored by this school? Here we had best deal with an actual situation, since a rethinking of conditoning theory is essential to our understanding. When the dog is first placed in a harness there is aroused an attitude of apprehension which expresses itself in a generalized postural adjustment. The dog does not 'know' from what quarter to expect punishment or reward. He is prepared for a wide range of possibilities. This tonic posture is active when the combination of bell (s_1) and shock (S_2) is first applied and it becomes r_1 in the resulting reaction-pattern r_1R_2. Since lifting the foot (R_2) is already connected with S_2 (unconditioned shock stimulus) there is a tendency for the postural readiness or apprehension (r_1) to be associated with the remaining stimulus s_1 (bell). But this s_1-r_1 connection does not remain unmodified by the S_2-R_2 connection, for under repetitive stimulation the posture of generalized apprehension is early changed into specialized adjustment in one part of the body. The pattern of anticipatory tension is now localized in the leg to be stimulated, and the dog turns his head in that direction. This qualitative and quantitative shift in r_1 may be accomplished without special reference to s_1. What probably happens is that r_1 persists longer in the part of the body making R_2 and this differential posture carries over to the application of the next conditioned stimulus.[9] Since the tonic factors outlast the phasic factors, it will at first be the only reaction to be connected with the stimulus-complex when S_2 (shock) is withdrawn. But since r_1 has been built up by training to be anticipatory of R_2 and since s_1 can now activate r_1 prior to the occurrence of S_2, something like R_2 may follow the s_1-r_1 connection. Thus while s_1 (the bell) was at first only connected with the apprehen-

[9] One of the strongest arguments in favor of this 'set' analysis is the fact that 'conditioning' occurs most easily when conditioned precedes, rather than coincides with, unconditioned stimulus.

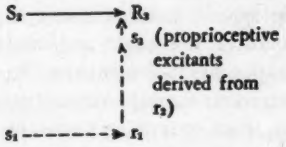
sive posture or set, it now carries to R_3 (conditioned foot withdrawal). By hypothesis, this connection is not direct, as indicated conventionally in Diagram 1, but indirect, as indicated in Diagram 2. In this scheme, the first effect of the postural adjustment (r_1) is to lower the threshold for R_3 (the phasic response) via proprioceptive afferents to the motoneurone field of R_1.

Diagram 1

Conventional C-R formula

Diagram 2

Role of tonic response (r_1) in conditioning



Later, if r_1 is sufficiently intense and if the S_2 it anticipates is absent, r_1 can itself supply a full substitute stimulus (s_1) through its proprioceptive afferents so that a weak approximation of R_3 may occur.

This unorthodoxy is not without support. Conditioned responses frequently possess a configuration quite different from the unconditioned response on which they are based.[10] Tolman[11] has indicated that many facts deriving from conditioned-reflex experiments cannot be adequately described in terms of the simple Pavlovian doctrine of stimulus substitution; Hilgard[12] writes similarly that "the dynamics of the conditioning process require either the abandonment of the simple formula, or its supplementation with other concepts;" and Guthrie[13] has mentioned the necessity of adding the animal's attitude or set as an organizing principle in conditioning experiments. Various investigators[14] have noted postural accompaniments of conditioning, and there is some evidence that these may precede the conditioned (phasic) response. The fact that a trained animal frequently begins to salivate when placed in the harness seems to indicate that a postural set may itself arouse a conditioned response. More crucial information would be provided by an experiment in which an attempt is made to shift the phasic response by shifting the postural adjustment or set.

It should be emphasized that we have recast the conventional C-R formula only as a means to an end. Our major interest has been to show the possible mechanism by which the tonic aspects of a patterned reaction may be sufficiently disassociated from the phasic aspects to become antecedent to, and anticipatory of, it. Whether this analysis can be extended to the more subtle varieties of prepared response is discussed in the next section.

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[10] E. C. Weaver, The upper limit of hearing in the cat, *J. Comp. Psychol.*, 10, 1930, 221.

[11] C. C. Tolman, *Theories of Learning in Comparative Psychology*, 1934, 317-408.

[12] E. R. Hilgard, The nature of the conditioned response: The case for and against stimulus-substitution, *J. Exper. Psychol.*, 20, 1936, 366-385.

[13] E. R. Guthrie, *The Psychology of Learning*, 1935, 1-258.

[14] Cf. in this connection, M. A. Wenger, A criticism of Pavlov's concept of internal inhibition, *Psychol. Rev.*, 44, 1937, 297-312.

(3) Set is essentially a central expression of the limiting and directing effect of tonic upon phasic response-aspects of the total reaction-pattern. In ascribing to the tonic response-factor a governing effect in all types of patterned reaction, our explanation of set enters its most difficult phase. If different neuromuscular activities always terminated in different effector-mechanisms, it would follow that differential patterns of tonic contraction could 'set' the part concerned for phasic response by lowering the thresholds of neurones in the final common path. Something like this may well occur in specific motor reactions. But there are many sustained intra-neural sequences of thought and memory where effector specificity is apparently impossible to demonstrate. To include the determinations which carry the organism through such activities as finding the solution to a mathematical equation or reviving a series of related experiences, however, we must think in terms of central effects.

Tonic contractions differ in their relative intensity and extensity, depending on the phasic acts which they sustain. Thus the tonic aspect of the conditioned foot-withdrawal is presumptively localized mainly in those muscles utilized in the fact. The set to add, on the other hand, is represented by a pattern of tension of wide extent but little intensive differentiation. These patterns do not stand for two mutually exclusive types of reaction; rather do they represent extremes of a scale, with central effects determined accordingly. Thus, when the postural pattern is highly concentrated in muscles which are to be utilized in the phasic act, the effect may be so marked that phasic response often begins without waiting for the appearance of the remainder of the stimulus-pattern. The runner who gets 'set' in response to a verbal command (s_1 in the conditioning analogy) shows a tendency to start before the gun is fired; the driver gets his car under way before he actually sees the green light; the dog lifts his foot before the shock is applied. In these cases, by hypothesis, excitation of tonic origin (either from the extra-pyramidal system or proprioceptive afferents from the muscles) is so intense or so highly localized that motor neurones in the final common path discharge to produce the phasic response. If, however, excitation of tonic origin is not sufficiently intense, and especially if its effects are not localized at some specific motor outlet, it can still act differentially to lower thresholds of higher level neurones so that these neurones are prepared to respond quickly and selectively to excitation of other origin.

Let us suppose a particular cortical pattern to be involved during the addition of numbers. This pattern is originally organized by the visual

symbols and under repetitive stimulation it becomes a *dominant*,[15] capable of summating formerly unrelated afferent impulses. The majority of such impulses are derived from the proprioceptive field. Increments in the neural excitation occasioned by an increased attentive posture flow toward the cortical region of greatest instability (the dominant) and further intensify its gradient. Thus a channelization of proprioceptive energies is created and a facilitative effect develops for the next phasic 'adding' pattern which the cortical neurones must sustain. All that remains now is for some part of the original stimulus-complex (verbal instruction or symbol) to capture this pattern of centrally channelized proprioceptive excitation (which is the set to add), and the cerebral dominant may be established prior to the application of the number sequences. According to this theory, the set to add is derived in the same way as the set to expect shock in conditioning experiments; *i.e.* when set to add, the organism takes a general attentive posture and this 'tonic' excitation is directed by the 'conditioned' instruction to arouse the cerebral dominant which is basic to 'phasic' adding activity.

(4) Tonic selection of phasic response holds for all types of reactions and for all levels of integration. The argument outlined above takes on body as we survey how the proprioceptive-tonic selection of exteroceptive-phasic response may occur at different levels of neural integration. When the skin on the flank of the spinal dog is stimulated, exteroceptive impulses flow into the spinal cord and produce a scratching response. However, the motor units which respond to these impulses are already under the influence of proprioceptive excitants emanating from the associated muscles; the exteroceptive impulses produce their effects upon this background and in accordance with it. Since these volleys are stronger and more nearly simultaneous than the proprioceptive excitants,[16] they may arouse the motor neurone field to full discharge—but not without the aid of the proprioceptors. If a muscle is de-afferented so as to lose its tone, formerly effective exteroceptive impulses cause little or no response. But the proprioceptive impulses do more than merely summate with any exteroceptive impulses which find their way to motor units under their dominance. They exercise a certain restraint and determination over the effectiveness of the enteroceptive impulses as well. Thus, if the position of the limb of our spinal dog were such that the muscles were dominated by inhibitory rather than

[15] For more detailed discussion of Ukhtomski's principle of the dominant, cf. G. L. Freeman, *An Introduction to Physiological Psychology*, 1934, 427-429.

[16] It will be recalled that in maintaining the normal tone of a muscle, only a few fibers are acting at a given instant, and so there is a weak but continuous discharge centrally of the proprioceptors.

excitatory proprioceptive impulses, the scratch-response will not be evoked. This is the situation when the same external stimulus gives first flexion and then extension of the leg.

Reactions integrated at higher neural levels indicate similar effects. Phasic responses to painful stimulation are very specific if the neuromuscular apparatus is prepared for the exteroceptive excitant, otherwise the typical generalized startle-response is made. Voluntary finger-oscillation begins as a pattern of exaggerated co-contraction in the antagonistic muscles. This 'warming-up' stage is essentially a function of the tonic system. Perceptive activities have a large postural component; selective reaction to a complex environmental field involves a general increase in reactivity and augmented tensions in muscles serving to place the end-organ appropriately. The occurrence of these reactions is generally recognized by the fixed gaze of the look-out at sea, the inclined ear and cocked head of the eavesdropper, and the concentrated expression of the tea-taster. These postural responses contribute to efficient sense impression; by hypothesis this is due to the channelization of proprioceptive excitation in specific cortical centers, thereby rendering these centers dominant in intracerebral fields.

The foregoing argument can be carried over to the reintegrational sequences of memory. One may take the set to recall the historical events connected with visually presented symbols, or one may associate these same symbols with different blocks in one's street of residence. Word-series will arouse different responses depending upon whether the subject is set to give subordinates, opposites, or synonyms. The 'control' in these controlled associations seems to be due essentially to a channelization of proprioceptive energies into highly limited cerebral fields. In thinking, the relational postures which mean "now I have it," "that is irrelevant," etc., are equally effective. While it is a long step from psychological description to physiological principle, we can probably regard the general thought-problem as involving a particular pattern of cortical stresses which is maintained by proprioceptive excitation. Work has already indicated that shifts and changes in these stresses are reflected in tissues under the dominance of the tonic reaction-system.[17]

In states of excited emotion, where the organism is functionally decorticate, the primary part of the reaction-pattern is tonic in character. Instead of highly specialized postural sets, the entire striate musculature is thrown into a state of heightened activity. This is accompanied by widespread visceral and humoral changes, with consequent central effects unchannelized.

[17] T. M. Abel, Attitude and the galvanic skin reflex, *J. Exper. Psychol.*, 13, 1930, 47-60.

Lack of specific connection between exteroceptive excitant and phasic response is indicated in the studies of emotional expression. It is significant that the only basic physiological distinction between emotional states is the tension or relaxation of the postural substrate. In excited emotion thalamic mass-discharge prevents control over phasic S-R relations. We look to this region of the basal ganglia for centers mediating differential postural adjustment and we find them in the striatal-tectal complex.

(5) Qualitative and quantitative character of the tonic response-segment varies with the similarity of the phasic responses it sustains. Having indicated that tonic selection of phasic response is apparently a very common phenomenon, it is important to add that not all tonicly derived sets are equally limiting. There are all degrees of specificity in the phasic responses which different tonic sets anticipate and carry through. In general, the more diffuse the postural effect, the more phasic acts it will support in common. This hypothesis can be tested experimentally. Suppose an animal so trained by the conditioning technique that the following stimulus-response sequences hold.

UCS cs_1	..... R_1r (left hind foot)
UCS cs_2	..... R_1r or R_2r (right or left hind foot according as USC is applied)
UCS cs_3	..... R_1r or R_2r or R_3r (right or left hind foot or right front foot according as UCS is applied)
UCS cs_4	..... R_1r or R_2r or R_3r or R_4r (right or left hind or front foot according as USC is applied)

In this situation the unconditioned shock stimulus (UCS) may be applied to any one of the four limbs, depending upon the particular conditioned stimulus (cs) used. Thus cs_1 becomes the signal for only one phasic response(lifting of the left hind foot), cs_2 for one of two, cs_3 for one of three, and cs_4 for one of four possible responses. According to hypothesis, the postural set (r) should show quantitative and qualitative differences according as it anticipates phasic reaction in one particular limb or in one of several different localities; *i.e.* tonic adjustments would be more diffuse and generalized for cs_4 than for cs_1 since cs_4 has to encompass more possibilities.

Something of this kind seems to hold quite generally. Thus there may be a general mathematical determination which supports use of all four fundamental processes or the specific set to add. In performances of this

order we should not expect qualitative differences between the postural accompaniments of these two determinations, though we might find greater tension-increments for the more general and non-specific determination.

(6) Specificity of phasic response to stimulation depends upon an ability to control and check shifts in the expression of tonic response-factors. According to our hypothesis, tonic factors in response develop through phasic reactions and in turn remain to induce and determine the character of subsequent phasic response. It is only as we neglect one part of this mutual relation that our experimental results become ambiguous and difficult of interpretation. Suggestions leading to greater specificity of phasic S-R relations through control of tonic bodily trends include:

(a) Make sure that the total experimental situation (including instruction and occasional setting) is held rigidly constant; otherwise unnoted variations in some aspects of the situation may become conditions for shifting the tonic set and hence the phasic response-segments observed.

(b) Allow sufficient time to lapse between the introduction of the subject into the experimental situation and the beginning of systematic observation; otherwise, residual effects of previous stimulation will be so marked as to upset and change the phasic reaction-pattern which is to be studied.

(c) Counter-balance all variables whose relation is being studied; otherwise bodily trends which are a function of time and repetition will not be eliminated and the data may show mainly the effect of such continuous postural adjustments as underlie fatigue, practice, boredom, expectation and apprehension.

(d) Employ some systematic check on the tonic aspects of response; otherwise uncontrolled effects of attitude and set will pass unnoticed.

The historical controls in psychological experimentation, such as are found in nonsense-syllable learning and by practice in general, have contributed much which is in line with the first three suggestions. Beyond all these, however, the psychologist must go to check upon the factors which actually govern the performance under survey. Reports by the trained observer provide such a check upon any reformulations which his task undergoes during the period of observation. But since many of our psychological problems are now attacked with naïve human subjects and lower animals, some other methodology will need to be developed in order to check on bodily trends which are at variance with the factors under investigation and control. Here we must turn to one or more of the bodily indicators, such as the galvanic skin-response, blood-pressure changes, and muscular action-potentials. It is known that any one of these processes will reflect quantitative changes and shifts in the mobilization of energies of tonic origin, though the relative advantage of each in special situations is a mat-

ter for extensive future research. Perhaps a mass recording of these processes in a variety of experiments will give a clue to their differential use in diagnosing the qualitative as well as quantitative shifts in neuromuscular action and the central effects which tonic response-segments sustain.

If our analysis of the problem of set has served its intended purpose, more questions have been raised than answered. Positive statements have been made, not in the spirit of dogmatic assertion, but in order to clarify the theory. It should be emphasized that the problem of set is a subject for experimental study, and that our hypothesis must be considered not so much by the pertinent facts which it now commands as by the lines of attack which it opens. The more important of these include (1) a new survey of conditioning phenomena, with adequate attention to the total postural adjustments involved, (2) a study of the modulating influence of tonic excitation on phasic reaction-sequences by combining stereotaxic stimulation with action-potential recording in animals, and (3) a check upon the relative efficiency of peripherally registered processes as indicators of tonic bodily trends.

In conclusion we would mention certain qualifications which are recognized in regard to the views set forth above. While giving particular attention to the influence of tonic over phasic response-factors, it is conceived that the relationship is mutual. Tonic sets do not arise *de nouveau* as determiners of phasic reactions. They are residues and originate out of previous phasic responses.[18] Furthermore, the notion of a division of labor within the total reaction-pattern sets up no hard and fast dichotomy; 'tonic' and 'phasic' may be purely relative terms, with a response-segment which is phasic in one performance serving a more static function in another. Dissociation of these 'elements' is likewise an artifact of analysis, as is any implied disjunction between functional course and initiation. The tonic factor may anticipate the phasic factor in time, but it always runs along with and is inevitably bound up with the entire reaction sequence. A momentary cross-sectional sampling of performance such as is popular with the neo-behaviorists, is not particularly informing as to the operation of our hypothesis. For crucial evidence we must return to the longitudinal type of survey used in the study of thought processes.

[18] The mechanisms involved here cannot be envisaged fully at the present time, though cerebral architecture suggests the importance of transcortical stimulation. The extra-pyramidal field (which is presumably associated with tonic bodily effects) covers a vast cortical territory and almost any specific connection between sensory and 'true' motor area could 'set up' this field and so influence subsequent transient phenomena.

Summary

The term 'set' is found so often in psychological writing that we forget that it has never been adequately defined. As used quite generally, the term refers to those vaguely defined organic states, conditions or processes which are antecedent to some overt reaction-pattern and which run along with, and so exercise a selective and determining influence upon, that reaction. Descriptive characterizations of set phenomena have been made in terms of both mental organizations and behavior, but there is a tendency for all writers to regard the problem as fundamentally neural. The present report constructs a working hypothesis of set in terms of postural responses and central nervous function. After indicating that all reactions involve both tonic and phasic response-segments, the hypothesis is developed that 'set' is a central expression of the limiting effect of proprioceptive-tonic activity upon exteroceptive-phasic activity. It is shown that at any given moment the organism is exposed to a great mass of internal and external excitants and that without some internal regulation there would be an indiscriminate diffusion of afferent discharge throughout the nervous system. This regulation is provided, by hypothesis, through a narrowing of the afferent flux into prepared adjustor and effector channels. On the one hand, proprioceptive excitation sustains selected exteroceptive excitations in the central apparatus; on the other hand, tonic contraction underlies phasic contraction. The mechanism by which specific postural adjustments or sets are developed and connected with external stimuli is suggested and a number of relevant questions are laid open to experimental attack. When the work of testing these propositions is finally completed it may be possible to regard the concept of set as physiologically superfluous and speak in the more ultimate terms of neural facilitation, lowered thresholds or altered potential-gradients. Until that time, however, its use does serve to hold together phenomena having a common matrix. The possibility of checking the government of psychological performance by means of related physiological processes is basic to more refined control in experimental situations. Ultimately we should expect that an increased knowledge of the mechanisms of set would result in a greatly improved methodology.

SHAPE AS A FUNCTION OF THE VECTOR-FIELD

By WILLIAM DILLARD ORBISON, State Hospital, Osawatomie, Kansas

With the advent of Gestalt psychology in 1912 a new theory of perception was enunciated which had as its basic principle the assumption that the properties of a given stimulus are determined by the total situation or Gestalt.[1] Since then the results of numerous studies have established the validity of this assumption.[2] Perceived brightness, form, velocity, and color have all been shown to be determined by the stimulus-pattern in which they are found.

For the Gestalt theory to have predictive value it was necessary for it to show how the total situation determines the activities and properties of the parts within it; *i.e.* to determine the laws of organization and to state them in such a manner that the facts of psychology can be deduced from them. This problem has been solved in various ways. Köhler[3] has set up a psychophysical theory of physiological Gestalten, Koffka[4] has postulated forces of organization, Lewin[5] has ordered Gestalten to a system of constructs based on topology, and Wheeler[6] has deduced a number of laws descriptive of the dynamics of organization. All of these methods have proved useful, but, with the possible exception of the topological concepts, they are either too general, or they lack the systematization necessary to lead to deductions of a quantitative sort.[7]

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[1] The author wishes to thank Dr. J. F. Brown for suggesting the problem of this paper and for help in preparing the manuscript.

[2] G. W. Hartmann, *Gestalt Psychology*, 1935, esp. Chap. 6, The phenomena of visual perception, 93-131.

[3] W. Köhler, *Die physischen Gestalten in Ruhe und im stationären Zustand*, 1920, 173-248.

[4] K. Koffka, *Principles of Gestalt Psychology*, 1935, 38-145.

[5] K. Lewin, *Principles of Topological Psychology*, 1936, 1-231. Before Lewin developed the topological psychology he had, in collaboration with Sakuma, suggested treating position in the visual field in a fashion similar to the one developed in this paper. K. Lewin and K. Sakuma, Die Sehrichtung monokularer und binokularer Objekte bei Bewegung und das Zustandekommen des Tiefeneffektes, *Psychol. Forsch.*, 6, 1925, 298-357.

[6] R. H. Wheeler and F. T. Perkins, *Principles of Mental Development*, 1932, 16-38.

[7] R. H. Wheeler, Postulates for a theory of education: A methodology for educational research, *J. Educ. Research*, 28, 1935, 187-195. In this paper Wheeler gives three stages in the development of laws: first, the prediction that the event will occur at all; secondly, the prediction of the direction of the process; thirdly, predictions of a quantitative nature. He states that his own laws are still in the first and second stages of development.

In the recent paper by Brown and Voth[8] a theory is advanced which makes possible quantitative predictions about the properties of visual Gestalten. In their theory visual experiences are ordered to a visual field, and vector field-forces, which are functions of physical variables, are postulated to account for the interactions taking place. The effect of the first of these, the cohesive field-forces, is to attract objects and cause them to flow together. The other field-forces, the restraining field-forces, describe the characteristics of the field which give stability to the objects in the field. Their direction is opposite that of the cohesive field-forces. The strength of both field-forces at any point is a function of time, space, and the distribution of intensity in the field. If the combined cohesive field-forces are designated by ΣC and the combined restraining field-forces by ΣR, three limiting field equations are possible:

$$(1)\ \Sigma R > \Sigma C$$
$$(2)\ \Sigma R = \Sigma C$$
$$(3)\ \Sigma R < \Sigma C$$

From this theory Brown and Voth made the following predictions. When the first of these conditions is realized, the field is chaotic, and the phenomenon classified as autokinetic movement occurs. The second condition gives rise to stable configurations, in which Gestalt properties may be deduced from the vector-field. When the third condition is realized, movement will occur. Brown and Voth concerned themselves experimentally only with the third equation, and by it they predicted the path of seen movement from the structure of the visual field. Their successful treatment of both real and apparent movement has led to the present study which follows their suggestion that many of the Gestalt phenomena of stable configurations can be predicted from the equation (2), where $\Sigma R = \Sigma C$.

Vector-Field Theory for Shape in Stable Configurations

If the Gestalt factors of stable configurations are to be accounted for by the posited field-forces, it must be possible in stable configurations to demonstrate their action experimentally. This could be done by showing their effect on objects introduced into the field, just as the presence of electric field-forces is detected with pith balls, and of magnetic field forces by the deflection of a magnetic needle. If it is possible to predict changes in the shape of geometrical figures introduced into stable configurations from the theory of the vector-field, the thesis may be looked

[8] J. F. Brown and A. C. Voth, The path of seen movement as a function of the vector field, this Journal, 49, 1937, 543-563.

on as verified. The present study characterizes with regard to the theory the vector-fields set up by geometrical patterns, and then seeks to verify the theory experimentally by predicting changes in the shapes of introduced objects.

In order to treat geometrical figures as vector-fields of cohesive field-forces, it is necessary to suppose certain relations between the field-forces and the properties of the objective stimulus-pattern. If two objects are brought into the visual field, they will immediately be acted upon by cohesive and restraining field-forces whose magnitudes are functions of the physical properties of the objective stimulus-pattern. In this paper the cohesive field-forces are treated solely as functions of the spatial separations between the objects in the field. They are assumed to increase as the spatial separations between the objects decrease. Mathematically, the relation may be represented by the formula: $\Sigma \mathbf{C} = K \cdot f[1/(C + d^n)]$, where d is the spatial separation, K and C are undetermined constants, and n is a constant not definitely determined but positive and equal to or greater than 1. If the two objects are seen as a stable configuration, by definition the $\Sigma \mathbf{R} = \Sigma \mathbf{C}$. This does not, however, mean that the two objects have not been attracted toward each other. Rather, it means that the two objects have been attracted toward each other until $\Sigma \mathbf{R} = \Sigma \mathbf{C}$.[9] Such an interpretation demands that the restraining field-forces increase as the object is displaced from its position. Therefore, it is further assumed that $\Sigma \mathbf{R} = K_1 \cdot f(d_1)$, where K_1 is an undetermined constant, and d_1 is the displacement of the object from its position. These two assumptions concerning the magnitudes of the field-forces will be used to determine the distribution of the forces in the vector-fields set up by geometrical patterns. The effect of other factors, such as size and intensity, on the magnitude of the forces will be discussed later. Previous studies show that the shapes of objects in the visual field are also dependent upon muscular pattern[10] and 'experience.'[11] However, in this initial study certain delimitations of the problem are necessary. Muscular pattern and 'experience' are assumed to have been held constant, and only two-dimensional patterns are considered.

The mode of attack indicated leads to the assumption that a figure introduced into a structured field will be distorted in the direction of the *positions of cohesive equilibrium* until equilibrium is reached, just as a

[9] In a special case the objects might be so placed that no displacement would be necessary for equilibrium to ensue.

[10] J. Fröbes, *Lehrbuch der experimentellen Psychologie*, I, 1923, 268; D. M. Purdy, The structure of the visual world: Space perception and the perception of wholes, *Psychol. Rev.*, 42, 1935, 399-424.

[11] H. A. Carr, *Introduction to Space Perception*, 1935, 402-403.

magnetic needle in a magnetic field will be rotated until equilibrium is reached. An example will serve to introduce the concept of position of cohesive equilibrium, which is useful in describing the vector properties of the fields. If three parallel lines of the same size and intensity are introduced into the visual field so that the interior line is midway between the two exterior lines, it will be in a position of equilibrium because the cohesive field-forces acting on it from the other two lines will be equal and opposite; the exterior lines will be moved inward until $\Sigma C = \Sigma R$. If the center line is moved so that it is no longer midway between the two exterior lines, it will no longer be in a position of equilibrium and will be distorted toward the nearer line. (For the purpose at hand, the disequilibration of the two other lines can be neglected.) If the restraining field-forces were not acting, the interior line and the line nearest it would meet in a position somewhere between their objective positions. The positions which objects in the visual field would occupy, if it were not for the action of the restraining field-forces, will be called *positions of cohesive equilibrium*. They can be determined from the assumptions made above, as will be shown in the next paragraphs. It should be possible to find situations where the restraining field-forces are weak as compared with the cohesive field-forces. In such situations the distortions should be experimentally observable, and the present paper reports such cases.

Characterization of Geometrical Patterns as Vector-Fields

Three geometrical figures (Fig. 1), which will be referred to as *geometrical* fields, were used in the experiments. The first task is to determine the positions of cohesive equilibrium for these fields. Field *A*, a pattern of radiating lines, will be considered first. (In Fig. 1, *A* represents field *A*, and *A'* an enlarged section of *A* which will be needed for the deductions.) In Fig. 1 *A'* consider the two field radii 1 and 2. Let a_1 and a_2 be points on radii 1 and 2, respectively, at a distance *a* from the field center *C*. CX_1 is the line bisecting the angle between radii 1 and 2. The line perpendicular to radius 1 at a_1 and the line perpendicular to radius 2 at a_2 will meet at a point on CX_1, designated by b_1. The cohesive field-forces from radius 1 acting on the points on the line a_1b_1 tend to pull them toward radius 1 approximately along the line a_1b_1; *i.e.* the points are shifted very little from the perpendicular. The same argument applies to the points on the line b_1a_2 with respect to radius 2. However, the cohesive field-forces from radius 2 act to pull the points on a_1b_1 toward radius 2 along the lines perpendicular to it; the cohesive field-forces from radius 1 act in the same manner on the points on the line b_1a_2. These forces are stronger the closer

the points are to the respective lines. (These forces are indicated by arrows in Fig. 1 *A'*.) The resultant is a curved line, $a_1b'_1a_2$, joining a_1 and a_2. This line is the position of cohesive equilibrium between the points a_1 and a_2, and is to be regarded as a minimum stress position in the field. Since this position is perpendicular to the two radii at the points of contact, as more radii are introduced into the field, the position will approach as a limit the arc of the circle, for one characteristic of a circle is that it is

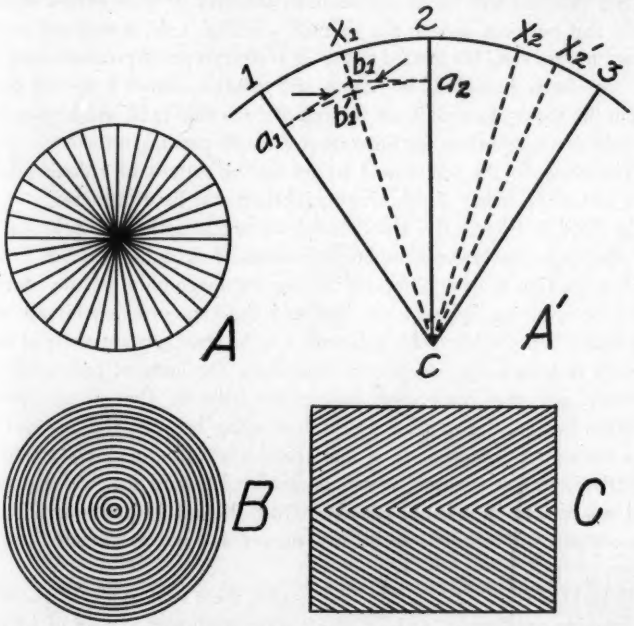

FIG. 1. THE GEOMETRICAL FIELDS USED IN THE EXPERIMENTS WITH THEIR RESPECTIVE POSITIONS OF COHESIVE EQUILIBRIUM

Positions of cohesive equilibrium are shown by the dashed lines. *A*, *B*, and *C* are the fields used. *A'* is an enlarged section of *A*.

perpendicular to each of its radii at the point of contact. Hence, the position, $a_1b'_1a_2$, will be treated as the arc of a circle. For the entire field the position is a circle with radius *a* and concentric with the field. Evidence for this is obtained from the fact that a circle placed concentric with the field is not distorted in shape. By letting *a* vary from *O* to *R*, the field radius, a set of positions of cohesive equilibrium, concentric with the field is ob-

tained. These are indicated by the dotted lines in Fig. 1 *A*.[12] The force with which an object is distorted toward these positions is inversely proportional to the distance of the object from the field-center, because the cohesive field-forces determining these positions are stronger the closer the object is to the field-center.

The bisectors of the angles between the radii, the lines CX_1 and CX_2 in Fig. 1 *A'*, are also a set of positions of cohesive equilibrium. A line in such a position will be in equilibrium; however, if it is moved slightly from that position, such as the line CX'_2 in Fig. 1 A', it will not be distorted toward CX_2 but toward radius 3. Thus, this set of positions need not be considered here because figures will not be distorted toward them. From the above example it can be seen that the field-radii will be positions of cohesive equilibrium for lines approximately parallel to them.

Therefore, in the vector-field set up by the pattern of radiating lines, two sets of positions of cohesive equilibrium may be determined.

In Field *B* (Fig. 1 *B*), a pattern of concentric circles, the principal set of positions of cohesive equilibrium consists of field-diameters. A line in such a position is in equilibrium because the magnitudes of the cohesive field-forces acting between the line and the concentric circles are at a minimum, and the forces are balanced; *i.e.* the resultants are of equal magnitudes and are acting in opposite directions. The locus of points halfway between any two consecutive concentric circles is also an equilibrium position because the cohesive field-forces acting between the loci and the two circles are balanced. If a line coincident with the locus is moved slightly from that position, it will be distorted toward the field-diameters and not toward its original position. Thus, this set of positions of equilibrium also will not be used because figures will not be distorted toward them.

In Field *C* (Fig. 1 *C*), a pattern of angles, there are two sets of positions of cohesive equilibrium, each of which is perpendicular to a set of parallel lines in the field. A line perpendicular to a set of parallels is in equilibrium because the cohesive field-forces acting between it and the parallels are balanced and their magnitudes are at a minimum.

From the theory developed it is expected that the restraining field-forces acting on objects introduced into the fields will be partially overcome by the cohesive field-forces of the fields, and that this will result in the distor-

[12] In Fig. 1, positions of cohesive equilibrium for the fields used are indicated by dashed lines. Since the theory requires that objects placed in these fields should be distorted toward these positions, the reader can determine for himself the nature of the distortions which should occur when a given figure is placed at a designated position in the field.

tions of the objects toward the positions of cohesive equilibrium determined for the fields. (In these experiments the reorganization of the positions of cohesive equilibrium resulting from the introduction of the objects into the fields is assumed to be negligible.) Thus, as shown in Fig. 2, a circle introduced into the field of radiating lines, so that the field-center is inside the circle but does not coincide with the center of the circle, should be distorted. The part of the circle nearest the field-center will have the greater curvature because this part of the circle will be distorted toward a posi-

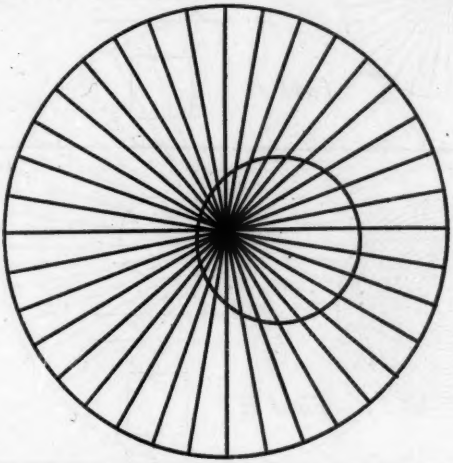

FIG. 2. DISTORTION OF A CIRCLE SUPERPOSED ON FIELD A
The field-center is inside the circle but not coincident with its center.

tion of cohesive equilibrium having a greater curvature than has the circle. The part of the circle diametrically opposite will have the lesser curvature because this part of the circle will be distorted toward a position of cohesive equilibrium having less curvature than has the circle. The prediction and experimental verification of the distortions which should result when given figures are introduced into the fields is reported in the next section.

EXPERIMENTAL VERIFICATION OF THE THEORY

Method, apparatus and subjects. The prediction of the distortions which should result when a given figure is placed at a designated position in the field was checked by superposing the figures on the fields and comparing the distortions reported by the group of *Ss* with those predicted by the theory.

To facilitate the placing of objects in the fields, both the fields and the figures to be placed in them were drawn in black India ink on white tracing paper. The figure was placed behind the field in a given position. Both figures were then placed immediately in front of a milk glass aperture in a specially constructed light box,

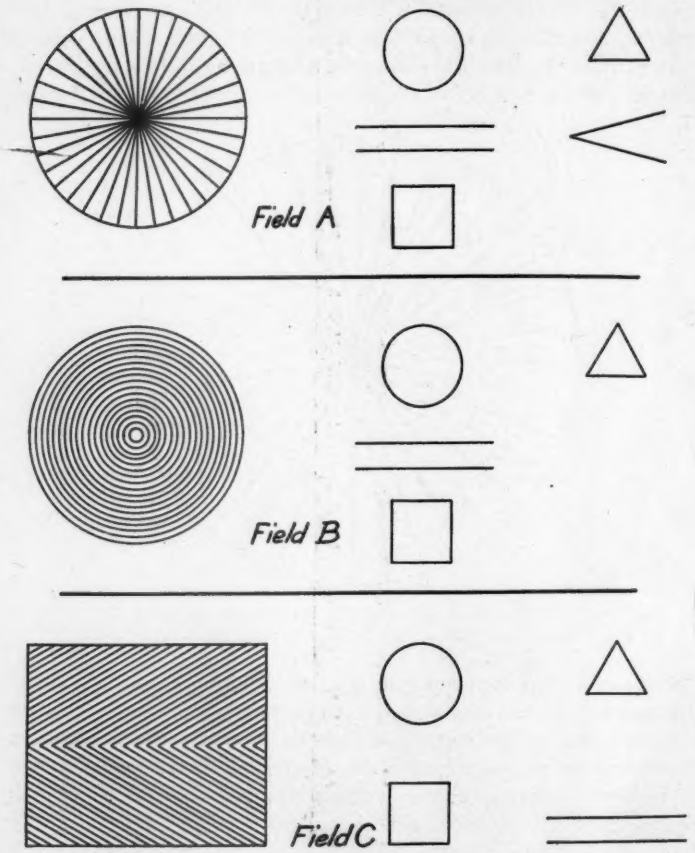

FIG. 3. THREE FIELDS WITH THE FIGURES WHICH WERE SUPERPOSED ON THEM
Both the figures and the fields are drawn to scale. See text for exact dimensions.

in which the intensity of illumination was controlled by a rheostat. In this position the figure was seen as if superposed upon the field. This method makes it possible to superpose figures on fields with great rapidity and to study the changes which take place in a figure when it is moved about in a field.

In the experiments to be reported, the field of radiating lines used was a circle, 10.8 cm. in diam., with 18 equally-spaced diameters drawn in it; the field of concentric circles consisted of 18 equally-spaced circles, the largest of which had a diameter of 10.8 cm.; the field of angles was a rectangle 12 x 10 cm., in which were drawn 60° angles with their corresponding sides parallel, and their vertices collinear and 0.6 cm. apart.

The figures superposed on these fields were the following: a circle of 4-cm. diam., parallel lines 7 cm. long and 1.2 cm. apart, a square 3 cm. on a side, an equilateral triangle 3 cm. on a side, and a 30° angle whose sides were 7 cm. long. The lines in all figures and fields were 0.5 mm. in width. Both figures and fields

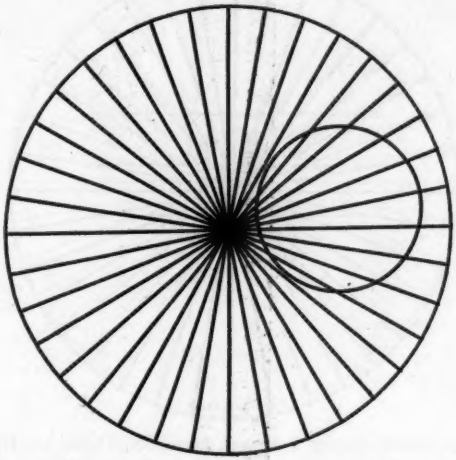

FIG. 4. DISTORTION OF A CIRCLE SUPERPOSED ON FIELD A
The field-center is outside the circle.

are reproduced in Fig. 3, which is drawn to scale. In the succeeding exposition it is to be understood that the figures introduced were those described above.

The *Ss*, some thirty in all, were faculty members and majors in the Department of Psychology at the University of Kansas. In the experimental situation *S* was shown a figure and told that he would be asked to describe the changes occurring in the figure when it was placed in the field. Since there was complete agreement among the *Ss* as to the direction of the changes which took place, the expression "verified by experiment" will be used to indicate that all *Ss* reported the predicted changes. Since these experiments are such as can be reproduced on the printed page, many of the resulting distortions are shown in the accompanying figures.

Prediction and experimental verification of distortions. The first experiments were concerned with the distorting effects of Field *A*. When the circle is superposed on the field so that the center of the circle coincides

with the field-center, the shape of the circle should not be distorted because the circle conforms to a position of cohesive equilibrium. No distortion was reported in the experiment. The case where the field-center is inside the circle but does not coincide with the center of the circle (Fig. 2) has already been discussed.

When the field-center is outside the circle (Fig. 4), the part of the circle nearest the field-center should have the least curvature, because

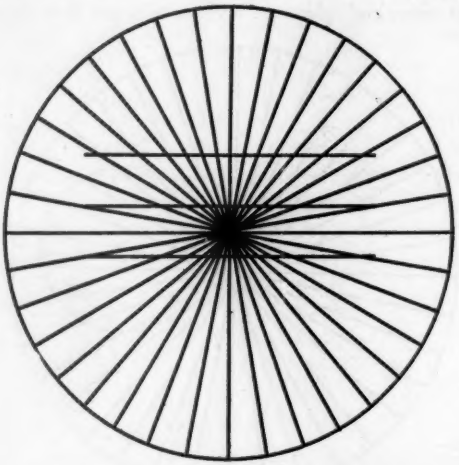

FIG. 5. DISTORTION OF PARALLEL LINES SUPERPOSED ON FIELD A

that part of the circle should be distorted to conform to a position of cohesive equilibrium having a curvature opposite that of the circle. The part of the circle diametrically opposite should have a greater curvature because, although it should have less curvature than has the circle when seen alone, it should be distorted toward lesser curvature, not opposite curvature. Then too, the distortion, without regard to direction, should be greater at the part nearest the field-center because the cohesive field-forces are stronger there. It should be obvious that these distortions all follow from the theory.

When the parallel lines are placed in the field so that the field-center is midway between them (the lower two horizontal lines in Fig. 5; Hering's figure), the distortion of the lines is in accordance with the theory that the lines should be distorted toward the positions of cohesive equilibrium in that the two lines are curved outward, and in that the distortion is

greatest at the parts of the line nearest the field-center. When the parallels are bisected by a field-diameter and the field-center is not midway between them (the upper two lines in Fig. 5), both lines should be distorted to conform with circles concentric with the field; the line nearest the field-center and the parts of both lines nearest the field-center should show the greatest distortion.

When the square is introduced into the field so that the field-center is

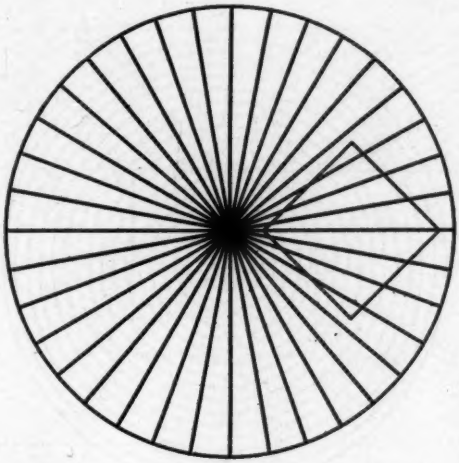

FIG. 6. DISTORTION OF A SQUARE SUPERPOSED ON FIELD A
The field-center is outside the square, and a diagonal of the square is collinear with a field-diameter.

inside the square, the sides should be distorted outward as are the parallels in Fig. 5. (This distortion can be better demonstrated by using a square smaller than the one superposed in this experiment.) If the centers of the field and the square do not coincide, the parts of the square nearest the field-center should show the greatest outward distortion. When the field-center is outside the square and a diagonal of the square coincides with a field-radius (Fig. 6), the two angles of the square bisected by the radius should appear to be greater than right angles, because the sides forming them, in being distorted toward the positions of cohesive equilibrium, should be distorted in a direction which will increase the size of the angles. The angle nearest the field-center should show the greater enlargement.

When the equilateral triangle is placed in the field so that the field-center is inside the triangle, the sides of the triangle should be distorted outward. If the centers of the field and triangle do not coincide, the parts of the triangle nearest the field-center should show the greatest outward distortion. When the field-center is outside the triangle and one of the altitudes of the triangle coincides with a field-radius, the angle nearest the field-center should appear to be larger than the other two. The ex-

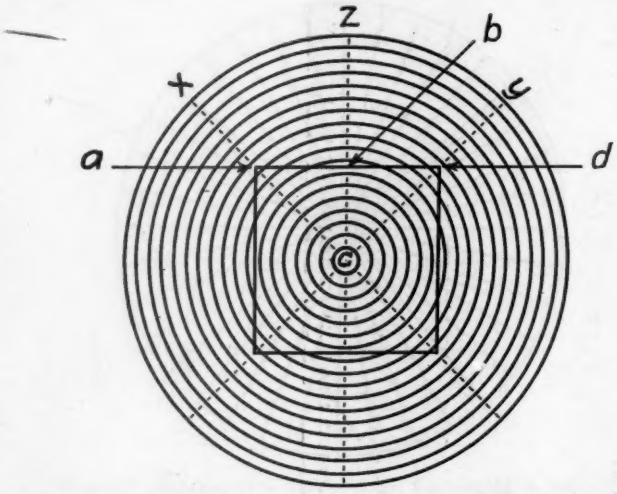

FIG. 7. DISTORTION OF A SQUARE SUPERPOSED IN A CONCENTRIC POSITION ON FIELD B
For purposes of representation the square shown in the figure is larger than the one used in the experiments.

planations for these distortions in the triangle are the same as for the similar distortions in the square.

When the 30° angle is so placed in the field that the field-center lies within the angle, the parts of the angle nearest the field-center appear to curve outward (Höfler figure).[18] This distortion is in complete agreement with the theory. In every case for all the *S*s and for other cases, the predicted distortions were verified by experiment. Consequently, the theory may be looked upon as verified, by experiment, for Field *A*.

The next experiments were concerned with Field *B*. Figures superposed

[18] Fröbes, *op. cit.*, 271.

on this field, if they are not in positions of cohesive equilibrium, should be distorted toward field-diameters (Fig. 1*B*). The distortion of the square in Fig. 7 is typical. The line *abd* appears to be bent inward at *b*. According to the theory, the line *ab* should be distorted toward the field-radii in the area *cxz*, and *bd* should be distorted toward the field-radii in the area *czy*. This will produce the apparent distortion of the line *abd* toward the field-center in the vicinity of *b*. Since the parallel lines, the circle, and the

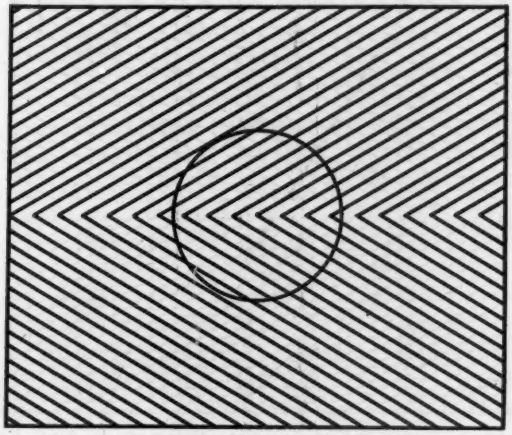

FIG. 8. DISTORTION OF A CIRCLE SUPERPOSED ON FIELD C
A diameter of the circle is collinear with the line joining the verticles of the angles.

triangle, when placed in the field, were all distorted in the direction predicted by the theory for all *S*s and all cases, the theory may be looked upon as verified for Field *B*.

The last experiments were concerned with Field *C*. Figures superposed on this field, if they are not in positions of cohesive equilibrium, should be distorted toward them. The distortions of the circle in Fig. 8 and of the square in Fig. 9 demonstrate the validity of this prediction. The circle in Fig. 8 has less curvature in the vicinity at the left than at the right. This is in complete agreement with the theory because the part of the circle at the right, if distorted toward the positions of cohesive equilibrium, will be distorted toward greater curvature, while the part of the circle at the left will be distorted toward lesser curvature. In Fig. 9 the angles at the left and right of the square appear larger than do the other two angles. A comparison of the positions of the sides of these

angles with the location of the positions of cohesive equilibrium shows that if the sides are distorted toward them, the angles will be increased in size. The other figure superposed on the field, the parallel lines,[14] the triangle, and the angle, were all distorted in the direction predicted by the theory for all *Ss* and in all cases.

The experimental results show that the direction of the distortion of objects introduced into geometrical patterns may be successfully predicted by characterizing

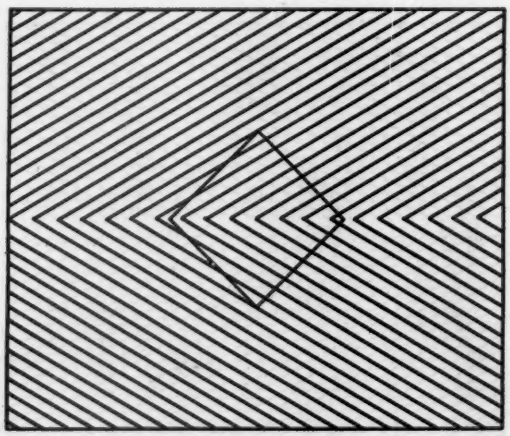

FIG. 9. DISTORTION OF A SQUARE SUPERPOSED ON FIELD C
A diagonal of the square is collinear with the line joining the vertices of the angles.

the patterns as vector-fields and by assuming that the field-forces are functions of the spatial separations. However, in making the predictions the effects of other physical variables have been neglected. Brown and Voth,[15] from their study of movement phenomena, suggest that the field-forces are functions of the time, space, and intensity distributions in the field. In a stable configuration time is a constant. Evidence that the intensity factor is almost a constant was obtained from the following experiment. When the intensity of a field was decreased by placing it on the light box and covering it with several sheets of plain white paper before the various figures were superposed on it, the figures were distorted almost as much as they were when

[14] The Zöllner Illusion can be constructed by superposing on the field two lines parallel to the line joining the vertices of the angles, so that the latter lies midway between the former. In this position the lines appear to meet in the direction opposite that toward which the angles, if considered as arrowheads, are pointing. This is the direction in which they should meet if they are distorted toward the positions of cohesive equilibrium. A study of the Zöllner figure or any of its modifications will show that the parallels are distorted toward the line perpendicular to the oblique lines crossing them.

[15] Brown and Voth, *op. cit.*, 549.

both the field and the figures were of the same intensity. When the intensity of the figure was decreased relative to the field, the distortion was somewhat greater than when the figure and the field were of the same intensity. It was also found that the dotted figures were distorted in the same direction, although not to as great an extent, as were the figures made of solid lines. One would suppose that both forces are functions of the sizes of the objects in the field. This can be shown to be true, but the effect is scarcely noticeable. For example, in Baldwin's figure[16] a dash half-way between two circles of different size is seen as being slightly nearer the larger circle. Thus, it seems logical to conclude, that in the fields studied, the magnitudes of the forces are determined largely by the spatial separations.

Conclusions and Implications for Further Research

The experiments confirm the vector-field hypothesis that the equation $\Sigma R = \Sigma C$ holds for stable visual perceptions. They do so by showing that this assumption leads to the experimentally verifiable implication that in stable visual configurations visual spatial values are distorted with changes in ΣC. This was demonstrated specifically by showing that the shapes of objects, superposed on two-dimensional geometrical patterns, are distorted in a predictable fashion by the action of the field-forces. In other words, the results show, not only that shape is determined by the total situation, but also that by ordering the total situation to a vector-field, it is possible to predict the shape which an object will have in a given total situation. Thus, the vector-field concept has been demonstrated as applicable to movement phenomena and to stable configurations. The results further confirm the hypothesis that the optic cortex functions as a vector-field.[17] The empirical determination of the relation between the field-forces and the physical variables used offers the possibility of a metrical treatment of the values of visual space. The theory advanced to account for shape is stated so that it is consistent with a field-theory covering the whole of visual perception. Its experimental verification shows the possibility of a generalized attack on the values of visual space.

No attempt has been made to predict the exact position of the introduced object relative to the field. This must wait until the theory is put in a more quantitative form. Since the theory in its present form has proved useful, an attempt is under way to put it into a more precise form and to expand it to include the phenomena of shape and position in both two- and three-dimensional space.

[16] E. C. Sanford, *A Course in Experimental Psychology*, 1898, 239.
[17] Brown and Voth, *op. cit.*, 561-562.

UNCONDITIONED STIMULUS-SUBSTITUTION IN THE CONDITIONING PROCESS

By W. J. BROGDEN, Johns Hopkins University

The usual conception of conditioning assumes that the conditioned stimulus (CS) substitutes completely for the unconditioned or original stimulus (US). The CS, however, is an effective substitute for the US only when it is followed by the US in its reënforcing capacity. Complete absence of the US brings a rapid extinction of the conditioned response (CR). Finch and Culler have pointed out that the US performs a dual function in the conditioning process.[1] First, it determines the character or pattern of response. Forelimb-flexion from a charged grid is clearly a function of the type of stimulus (electric shock) and also of its locus of application (forepaw). The CS takes over this function of the US only, that of evoking a definite behavior-pattern. Secondly, the US provides the incentive or drive needed to actuate this response. The CS does not substitute for this function of the US. Only constant reënforcement with the US maintains the excitatory tendency of the CS at a maximum. Dogs conditioned to forelimb flexion by these authors required a mean time of 14.9 periods for extinction when a buzzer (which during training sounded for the exact duration of the electric shock) was present and 4 periods when it was absent. The buzzer was neither an US nor a CS for flexion; at no time was it possible to elicit foot-withdrawal to the buzzer sounded alone. The buzzer had for a time taken over the second function of the US, that of providing the incentive or drive for response. The buzzer did not effectively take the place of shock in maintaining a consistent flexion to the CS, but did definitely retard the normal progress of experimental extinction.

It should be possible, however, to elicit a consistent CR to a CS, by substituting a reënforcing agent for the incentive function of the US. With neutral stimuli substituting for both of the functions of an original stimulus, the CR would both be evoked and maintained in the complete absence of the US. Such a response would be truly 'conditioned.' The present investigation was designed to see whether it is possible to substitute novel stimuli for each of the two functions of an US. In other words, once dogs

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[1] Glen Finch and Elmer Culler, Relation of forgetting to experimental extinction, this JOURNAL, 47, 1935, 656-662.

are conditioned with a bell to flexion from an electrically charged grid, is it possible to substitute another stimulus, *e.g.*, food, for the reënforcing function of shock? Furthermore, since food is the US for salivation, will the bell evoke a conditioned secretory response as well as a conditioned flexion response?

Experimental Procedure

Within a soundproofed, light-shielded test-room, with *E* observing through a one-way glass screen, left forelimb-flexion from an electrically charged grid was conditioned to the sound of an electric doorbell in eight mongrel dogs. With six of the animals (*Nos.* 1, 2, 3, 6, 7, 8), the duration of the bell was 10 sec.; with the other two (*Nos.* 4, 5) it was 2 sec. The shock, produced by a high voltage step-up transformer and made variable by suitable resistances in series, was given always at the end of the stimulus-period for approximately 1/10 sec. Conditioning proceeded rapidly with 20 trials per day as the test-unit. Only those conditioned flexion-responses which avoided shock were scored as positive. When one series of 100% shock-avoidance was obtained, every correct response thereafter was rewarded by a biscuit dropped from the food-box, shock being omitted altogether from the test situation. Three of the animals (*Nos.* 6, 7, 8) with a 10-sec. stimulus-duration were continued on shock as controls. The experimental animals were given the bell-food sequence for 1000 trials after the last shock reënforcement. In order to keep the factor of hunger as constant as possible, tests were made in the morning after a 17-20 hr. fast. In no case did any of the animals flex the left forelimb when given food alone. Food was therefore neither an US nor a CS for flexion. The duct of the left parotid salivary gland of the three experimental animals (*Nos.* 1, 2, 3) had been earlier made into a permanent fistula. By sealing a disk attached to a suitable hydraulic system over the fistula with wax, salivary secretion could be measured in a capillary tube on a millimeter scale above *E*'s screen in the test-room. Displacement of 1 mm. on the scale is equal to a volume of 0.00167 c.cm. The stimuli were electrically controlled. Measures of latency were made with a stop-watch. All of the customary precautions, such as variation in the time between stimuli to prevent temporal conditioning and elimination of possible clues from the operation of the apparatus were rigidly observed.

Results

Substitution of food for shock reënforcement of the conditioned flexion-response to bell was made with relative ease. Once shock was omitted there was some hesitancy on the part of all animals to eat, which in no case persisted beyond the tenth trial. As food continued to appear in the food-box after each flexion response to bell, there was a tendency for flexion to be of short duration or completely absent, replaced by the immediate turning of the head to the food-box in the anticipation of food. Also, there was a tendency for 'spontaneous' responses (flexion in the absence of bell) to appear, followed by turning of the head to the food-box. Since to none of these responses was food forthcoming, it was necessary in several cases to

return to reënforcement with shock in order to reëstablish a persistent flexion for the duration of the bell. Animal 1 required shock reënforcement on 4 different occasions; Animals 2 and 4 each required one reënforcement series; and Animals 3 and 5 were immediately successful, requiring no reënforcement with shock. The final pattern of response to bell was sustained flexion until the bell was silenced or until food dropped into the box, whereupon the animal turned and ate.

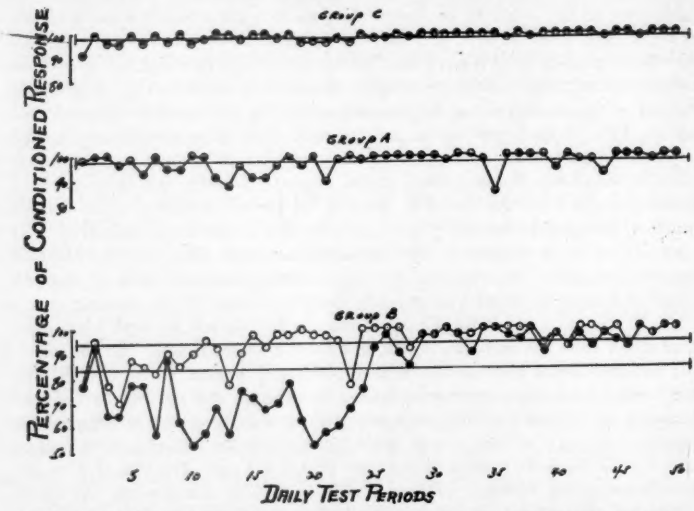

FIG. 1. THE DAILY MEAN SCORES OF THE PERCENTAGES OF CONDITIONED RESPONSES FOR THE DIFFERENT GROUPS OF ANIMALS

The straight line passing through each curve represents the mean group response for the entire experiment. Of the two scores for Group B, the filled circles represent the actual score, whereas the open circles represent the score of that group when the incomplete responses of Animal 5 are added to the actual score.

This behavior was consistent for 1000 trials after the last shock reënforcement. Animal 1 gave a mean for the 50 test-periods of 98.7% (S.E. = 0.57%), Animal 2 a mean of 97.0% (S.E. = 1.05%), and Animal 3 a mean of 97.4% (S.E. = 0.87%). These three animals, conditioned to a 10-sec. stimulus, are designated as Group A in Fig. 1, where the daily mean scores of this and the other two groups are plotted for each of the 50 test-periods. For Group A, the mean response for the whole series is 97.7% (S.E. = 0.50%). Animals 4 and 5, being conditioned to a 2-sec. stimulus, comprise Group B. They gave a mean score of 92.9% (S.E. =

1.55%), and 68.8% (S.E. = 4.04%) respectively, and a mean group score of 82.9% (S.E. = 2.30%). Animal 5 was very difficult to score, since many times its flexion-response was completed just before the end of stimulation. Such incomplete responses were never rewarded with food as they did not satisfy the response-criteria. If these responses are scored as positive, the mean response of Animal 5 becomes 95.3% (S.E. = 1.72%), a level more compatible with those of the other animals. Likewise the mean score of Group B is raised to 94.4% (S.E. = 1.11%). The daily means for this group are plotted in Fig. 1 in two curves: one in which Animal 5 is scored as the other animals; and another in which the incomplete responses are added to the actual score. Animals 6, 7, and 8 were continued with shock throughout the experiment, and with a 10-sec. stimulus, served as controls for Group A. These animals, comprising Group C, gave mean scores of 98.9% (S.E. = 0.33%), 98.9% (S.E. = 0.33%), and 98.7% (S.E. = 0.39%) respectively, and a mean group score of 98.8% (S.E. = 0.25%). If in Fig. 1 we compare Group A with its control, Group C, we find the experimental group to be less regular in response, the standard error of the mean (0.50%) being twice that of the control group (0.25%). The difference between the means of the two groups is, however, only 1.1% (S.E. = 0.56%) in favor of the controls. With a critical ratio of 1.96, the two groups do not differ significantly. Food, therefore, functioned here as did the buzzer in the experiment of Finch and Culler previously mentioned, but with complete effectiveness, since food maintained as high a level of flexion as did shock. The slightly greater irregularity of the experimental animals was probably due to variation in appetite. Shock is usually a more consistent incentive than food.

Group B under both scoring conditions of Animal 5 shows a great deal of irregularity (S.E. = 2.30% and 1.11%) and a much higher level of response in the last 500 trials than in the first 500, being 90.10% and 98.38%, and 69.44% and 96.36% respectively, the true score being given first in each case. Since the low level and irregularity of response was due, however, to Animal 5, whose behavior has been fully discussed earlier, it is most likely that animals conditioned to flexion with a stimulus of 2-sec. duration would respond as frequently and regularly when food was substituted for shock as when it was not.

The mean latency of response for Animal 1 was 2.17 sec. (S.E. = 0.03 sec.), for Animal 2, 3.93 sec. (S.E. = 0.23 sec.), and for Animal 3, 1.28 sec. (S.E. = 0.05 sec.), the mean for Group A being 2.49 sec. (S.E. = 0.08 sec.). No measures of latency were made on the two animals in Group B. In the control group, Animal 6 had a mean latency of 3.98 sec. (S.E.

= 0.01 sec.), Animal 7, 1.77 sec. (S.E. = 0.08 sec.) and Animal 8, 1.91 sec. (S.E. = 0.07 sec.). The mean for Group C is thus 2.55 sec. (S.E. = 0.06 sec.). The difference between the two groups is only 0.06 sec. (S.E. = 0.10 sec.) in favor of the control group. In spite of the tendency for the latency of Group A to increase toward the end of the series, the minute difference between the two means leads to the conclusion that latency of conditioned flexion to bell is the same, whether it is reënforced by food or by shock (see Fig. 2).

In answer to the second question of the investigation, we find that the three animals of Group A did salivate to bell, once flexion to bell was consistently reënforced with food. Salivation to bell occurred in the first period in which food was substituted for shock in both Animals 1 and 2, and in the second period in Animal 3. Conditioned secretion to bell per-

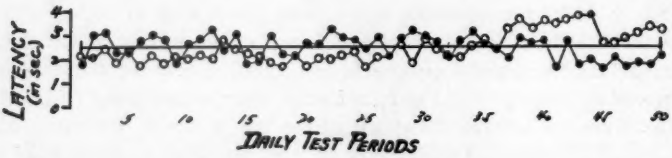

FIG. 2. THE DAILY MEAN LATENCY OF FLEXION
The open circles representing Group A and the filled Group C. Since the mean difference in latency of flexion is only 0.06 sec., the straight line represents the mean latency of response for both groups.

sisted for several periods when Animal 1 was returned to shock for retraining and never disappeared during the two periods in which Animal 2 was retrained with shock. Animal 3 needed no retraining with shock. For the 1000 trials, Animal 1 secreted to bell 72.4% (S.E. = 5.05%) of the time with a mean intensity of 18.75 mm. (S.E. = 1.97 mm.); Animal 2 salivated 43.6% (S.E. = 4.50%) of the time with a mean intensity of 13.12 mm. (S.E. = 1.88 mm.); and Animal 3, 78.50% (S.E. =3.68%) of the time with a mean intensity of 32.40 mm. (S.E. = 2.70 mm.). The mean percentage response for the group was 64.4% (S.E. = 3.17%) with a mean intensity of 22.00 mm. (S.E. = 1.23 mm.). In Fig. 3 the daily group scores of frequency of conditioned flexion, frequency of conditioned salivation, and intensity of conditioned salivation are plotted. The curves of frequency and intensity of salivation follow each other quite closely. They are both quite irregular and fluctuate widely around the mean. The mean difference in frequency of flexion and frequency of salivation is 33.30% (S.E. = 3.21%) in favor of flexion. Although during the 1000

trials bell was followed by food 977 times, bell never consistently evoked salivary secretion. When salivation did occur it was never of very large magnitude. If we examine the results for every animal we find that Animal 1 after the first 30 days gave 16 days of 100% response, the other 4 days in no case dropping below 80%. In the first 30 days, Animal 1 gave 100% response on 3 days only. Animal 2 gave 100% on only 3 occasions in the whole series. Animal 3 secreted 100% of the time on 20 out of 50 days, but only 8 of these were consecutive, from the 17th to the 25th days. Animals 1 and 3 gave a consistent salivary response at some time or another.

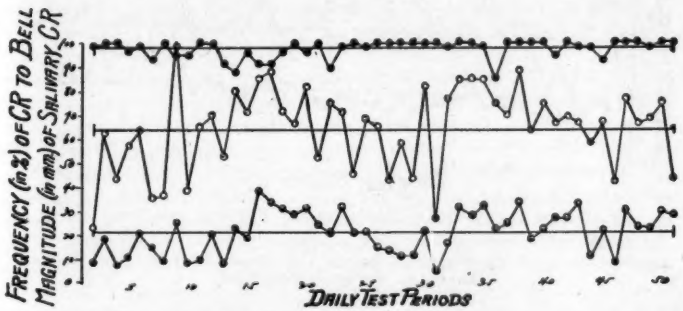

FIG. 3. THE DAILY MEAN RESPONSE OF THE THREE ANIMALS IN GROUP A
The filled circles represent frequency of conditioned flexion, the open circles frequency of conditioned salivation, and the half-filled circles intensity of conditioned salivation. One millimeter of secretion equals a volume of 0.00167 cm. The straight line passing through each curve represents the mean group response for the entire experiment.

Animal 2 never gave a very consistent response and the intensity was always variable.

The mean latency of conditioned salivation was 5.55 sec. (S.E. = 0.28 sec.) for Animal 1, 5.57 sec. (S.E. = 0.31 sec.) for Animal 2, and 3.89 sec. (S.E. = 0.21 sec.) for Animal 3. The group mean latency of salivation was 4.97 sec. (S.E. = 0.16 sec.). The difference between the mean latency of conditioned flexion and conditioned salivation was 2.48 sec. (S.E. = 0.10 sec.). In other words salivation when it occurred, began 2.48 sec. after flexion. In Fig. 4 the daily mean latencies of conditioned flexion and salivation for the group are plotted.

It is rather difficult to understand why these animals gave no consistent salivary response to bell, even though food followed bell 97.7% of the time. Several hundred trials of bell-food is usually sufficient to produce a consistent conditioned secretion to each stimulus-presentation. The experi-

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ment was continued to 1000 trials only to see whether conditioned salivation would become consistent. Flexion-failure to bell with subsequent lack of food was so rare that it can scarcely be said that conditioned salivation suffered extinction. Likewise the salivary mechanism of the animals cannot be considered pathologic, as they all gave a good unconditioned secretion and at one time or another, a consistent conditioned salivation. Nor can the several hundred trials of bell-shock prior to food substitution be considered effective in deterring the development of a conditioned secre-

FIG. 4. THE DAILY MEAN LATENCIES FOR FLEXION AND SALIVATION
The filled circles represent latency of conditioned flexion and the open circles represent latency of conditioned salivation. The straight lines represent the mean latency for the entire experiment.

tory response. There is no apparent reason why conditioned salivation to bell should be so irregular, inconsistent, and inferior to conditioned flexion.

DISCUSSION

The importance of the dual functioning of the US in the conditioning process must be emphasized. It has long been recognized that the sense organs, particularly the distance receptors, play an important rôle in conditioning by permitting the organism to anticipate and prepare for the action of the US. It is obvious that the number of stimuli which can be utilized as signals is almost infinite, particularly in the human where a written and spoken language is an immense reservoir for such stimuli. On the other hand the number of responses which an organism can make is definitely limited. Muscles and glands can respond only in so many ways, and the permutations and combinations of coördinated grandular or muscular activity are finite. The number of simple or unconditioned responses is still more limited and there can be no more unconditioned stimuli (US) than there are unconditioned responses (UR); but just as the function of eliciting a specific response can be taken over by a neutral stimulus, so can the function of providing the incentive for response be replaced by a novel stimulus. Any given response can be made to many stimuli and can be reënforced by different incentives at different times. For example, the human grasping with the right hand is conditioned to many stimuli and is reenforced in many ways. In the experiment just reported shock elicited flexion of the left forelimb. Once conditioning was established, bell evoked flexion. It would have

been possible to make many other stimuli appropriate signals by combining them with shock. Bell continued to evoke flexion when shock was omitted and food given at the end of the response. Food, neither an US nor a CS, replaced the reënforcing function of shock. At no time did food given alone call forth flexion. Flexion was therefore completely 'conditioned,' being elicited and reënforced by stimuli once neutral, long after the appropriate original stimulus had been removed from the experimental situation.

It is rarely difficult in a discussion of human behavior in terms of conditioning to define the stimulus or stimuli which evoke a given response, but it is almost impossible to isolate the unconditioned stimulus from the situation. The behavior of animals and infants is much more amenable to description in terms of the conditioned response, since the original stimuli are largely physiological in nature. In the development of behavior in the infant, neutral stimuli take over the function of the US of evoking a specific pattern of response. As long as these response-patterns are reënforced with the adequate US, the elements of the conditioned response-pattern are clearly discernable. As the infant develops, however, he begins to make responses to conditioned stimuli which are never reënforced by the unconditioned physiological stimulus. As an adult many stimuli evoke response, but little of this behavior is ever reënforced by the original or unconditioned stimulus. Yet the behavior patterns are essentially the same as when he was a child. What has happened to the US? It has been replaced by one of the many forms of social approval or disapproval. The incentive function of the US has been replaced by a social mechanism of approbation or censure, just as food replaced the reënforcing function of shock in maintaining forelimb flexion to bell. The more complex responses of humans can now be more easily discussed as conditioned responses. The CS stands out in any situation as the signal which elicits a given form of behavior. The incentive or drive which maintains the response to the CS is provided by the social setting and varies with the utility of the response. Reënforcement of response by the unconditioned physiologic stimulus is rare. In fact, responses to physiological stimuli are more often inhibited by the social setting. To receive social approbation and to avoid social censure is frequently more important than the immediate release of a physiological tension.

In a discussion of two types of CR, Skinner,[2] and Konorski and Miller,[3] agree to designate the usual Pavlovian CR as type S and the new CR (upon the formulation of which they disagree) as type R. The new type is one in which a response such as pressing a lever or flexion of the forelimb is followed by food. Skinner states that the response is evoked by no observable stimulus. The response occurs spontaneously and once it is reënforced, develops rapidly. Konorski and Miller evoked flexion by shock and gave food after flexion. Soon the animals flexed the forelimb when placed in the experimental situation to obtain food, shock being omitted thereafter. It is evident that flexion evoked by bell and reënforced by food is definitely related to the type R conditioned response, or to the so-called pseudo-type reported

[2] B. F. Skinner, Two types of conditioned reflex: A reply to Konorski and Miller, *J. Gen. Psychol.*, 16, 1937, 272-279.

[3] J. Konorski and S. Miller, On two types of conditioned reflex, *ibid.*, 16, 1937, 264-272; Further remarks on two types of conditioned reflex, *ibid.*, 17, 1937, 405-407.

by Skinner,[4] where lever-depression by the rat was reënforced with food only when the response occurred during stimulation by a light. It is certain that any response must be evoked by a stimulus of some sort, whether it is clearly apparent or not. It is also more probable that any response will be elicited by a CS than by an US, since organism-survival depends upon conditioning, and any organism with even a relatively short survival-period responds to many CS. It would seem that the type R CR will appear most frequently in the form in which it is represented in this experiment. A CR is formed (flexion), being evoked by a definite CS (bell). The CS elicits the CR in a situation where reënforcement is given by a new stimulus (food), one which does not itself evoke flexion. As Skinner says of his discussion with Konorski and Miller, "The question at issue is whether we may produce contraction of the pupil according to s—contraction→reënforcement, where (for caution's sake) the reënforcing stimulus will not itself elicit contraction."[5] Certainly bell—flexion→food fulfills Skinner's conditions, since food at no time was capable of evoking flexion.

Summary of Conclusions

(1) Substitution of food for the reënforcing function of shock in maintaining a consistent conditioned flexion-response to bell was made with relative ease in five animals.

(2) Once food effectively replaced shock, it kept conditioned flexion to bell at a mean response-level of 97.7% (S.E. = 0.50%) for Animals 1, 2, and 3 and at a mean response-level of 94.4% (S.E. = 1.10%) for Animals 4 and 5. In the first group (Group A), the bell was of 10-sec. duration; in the second group (Group B), it was of 2-sec. duration.

(3) The difference in level of response between Group A and its control group, Group C, consisting of three animals continued with shock for 1000 trials, was 1.1% (S.E. = 0.56%) in favor of the control group.

(4) The difference in latency of flexion between Group A and Group C was 0.06 sec. (S.E. = 0.10 sec.) in favor of the controls.

(5) Conditioned flexion to bell reënforced by food does not differ in frequency and latency of response from conditioned flexion to bell reënforced by shock in the animals studied.

(6) Since food given alone at no time evoked flexion in any of the animals, it was neither an US nor a CS for foot-withdrawal. Food, therefore, effectively substituted for the reënforcing function of shock, the US for forelimb-flexion.

(7) Group A, with salivary fistula, gave conditioned salivary secretion 64.4% (S.E. = 3.17%) of the time during the whole series of 1000 trials. The mean intensity of response was 22.00 mm. (S.E. = 1.23 mm.). Condi-

[4] Skinner, Two types of conditioned reflex and a pseudo-type, *ibid.*, 12, 1935, 66-77.

[5] Skinner, *op. cit.*, *J. Gen. Psychol.*, 16, 1937, 279.

tioned salivation occurred 33.30% of the time less than did flexion and its latency was 2.48 sec. greater than that of conditioned flexion. Conditioned salivation at different times, for short periods, was as consistent as was flexion, but in none of the animals did it persist for long. There is no apparent reason why conditioned salivation should be so inconsistent and inferior to conditioned flexion, particularly when bell was followed by food 977 times in the 1000 trials.

(8) The importance of the dual rôle of the unconditioned stimulus in conditioning as pointed out by Finch and Culler is emphasized.

(9) Conditioned flexion to bell reënforced by food is discussed as similar to the type R CR of Skinner, and of Konorski and Miller

THE PSYCHOLOGICAL EFFECTS OF BENZEDRINE SULFATE

By EDWARD C. REIFENSTEIN, JR., and EUGENE DAVIDOFF, Syracuse University

Benzedrine sulfate is a drug of considerable interest because of its stimulating action on the central nervous system. This property of the drug has been successfully utilized in medicine in the treatment of narcolepsy, post-encephalitic Parkinsonism, certain depressive mental states and similar conditions. Psychologists are employing the drug to secure additional data on the functioning of the human mind. In addition, benzedrine sulfate has an action on the sympathetic nervous system which we have previously referred to as a pseudo-sympathomimetic effect.

We have used benzedrine sulfate in normal subjects and mentally disordered patients since January 1936.[1] On the basis of this experience we are presenting a summary of the psychological responses in over 200 persons thus medicated. For purposes of description the reactions to the drug may be roughly divided into those relating to mental, to speech, to motor, and to affectual function. We recognize that this division is artificial, and that these responses are manifestations of a total individual reacting as a unit, and therefore cannot be evaluated except in relation to the total situation. In analyzing each case the previous mental status was used as a base line in evaluating the response.

We have characterized the action of benzedrine sulfate as variable, uncertain, unpredictable, and at times paradoxic.[2] From the psychological manifestations enumerated below the pattern of reaction of the central nervous system is readily seen to be similarly variable and paradoxic. To account for such variations we have suggested as a possible explanation (presented in detail in previous communications)[3]

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[1] Eugene Davidoff, A clinical study of the effect of benzedrine on self-absorbed patients, *Psychiat. Quar.*, 10, 1936, 652-659. E. Davidoff and E. C. Reifenstein, Jr., The stimulating action of benzedrine sulfate: A comparative study of the responses of normal persons and of depressed patients, *J. Amer. Med. Assn.*, 108, 1937, 1770-1776; A method of studying some of the physiologic actions of benzedrine sulfate, *J. Lab. & Clin. Med.*, 23, 1938, 700-711; Treatment of schizophrenia with sympathomimetic drugs: benzedrine sulfate, *Psychiat. Quar.*, 13, 1939, (in press); The results of eighteen months of benzedrine sulfate therapy in psychiatry, *Amer. J. Psychiat.*, (in press); E. C. Reifenstein, Jr., and E. Davidoff, Intravenous benzedrine sulfate as an antagonist to intravenous soluble amytal, *Proc. Soc. Exper. Biol. & Med.*, 38, 1938, 181-184; The treatment of alcoholic psychoses with benzedrine sulfate, *J. Amer. Med. Assn.*, 110, 1938, 1811-1813; The present status of benzedrine sulfate therapy, *N. Y. State Med. J.*, (in press).

[2] *Op. cit., J. Amer. Med. Assn.*, 108, 1937, 1770-1776; *J. Lab. & Clin. Med.*, 23, 1938, 700-711.

[3] *Op. cit., J. Lab. & Clin. Med.*, 23, 1938, 700-711.

that the response of a given individual depends on the interaction of a number of factors among which must be included: the physical, physiological and psychological status of the medicated individual; the several actions of benzedrine sulfate on the central nervous system, the sympathetic nervous system, and possibly on other components of the nervous system; and the factors introduced by the personality and interpretation of the investigator. The rôle of compensatory delayed reactions involving the parasympathetic as well as the sympathetic nervous system and the work of Pavlov and Sherrington must be considered in this connection. We have suggested that the drug renders a given personality more adaptable in a given environmental situation by increasing the number of available pathways of coördinate action.

Effects of benzedrine sulfate. In our experience benzedrine sulfate has produced the following effects.

(1) Stimulation of mental response: (a) Major effects. Acceleration of the stream of mental activity and increase in content of thought; increase in coördinate mental activity and general efficiency; increase in perception and alertness; increase in initiative; increase in attention and meticulousness; decrease in confusion; decrease in sleep requirement and insomnia; decrease in fatigue; decrease in reticence and improved performance on psychometric tests. *(b) Paradoxic effects.* Retardation of the stream of mental activity; inability to concentrate; forgetfulness; dullness; increase in fatigue, malaise and drowsiness; decrease in perception and alertness; decrease in attention; confusion; transitory delirium; tactile and other somatic hallucinations, paresthesias and anesthesias, heart consciousness, and generalized sensations of warmth.

(2) Stimulation of speech function: (a) Major effects. Increase in speech activity, disappearance of mutism, talkativeness to the point of loquaciousness; increase in accessibility with willingness to discuss personal problems, ventilation of conflicts, and elaboration of delusions and hallucinations; and increase in coherent and relevant speech. *(b) Paradoxic effects.* Decrease in speech activity; decrease in logical progression of thought with increase in incoherent and irrelevant speech.

(3) Stimulation of motor function: (a) Major effects. Increase in motor activity; increase in coördinate physical activity with increase in physical efficiency; tremulousness, restlessness, extreme hyperkinesis; and aggravation of convulsive seizures. *(b) Paradoxic effects.* Decrease in motor activity with decrease in physical efficiency.

(4) Stimulation of affectual function: (a) Major effects. Elevation of mood with cheerfulness, a feeling of well-being exhilaration, euphoria, elation; uncontrollable laughter; facetiousness; a state of ready irritability and increased irritation; increase in drive, urge and spontaneity; decrease in shyness; increase in confidence; hair-trigger-like reactions with sudden changeability of mood; agitation, impatience, impulsiveness, and self assertiveness approaching aggressiveness in some; aggravation of assaultiveness, and homicidal tendencies in mentally ill persons. *(b) Paradoxic effects.* Lowering of mood with depression or melancholy; uncontrollable crying; aggravation of suicidal tendencies; resentfulness; surliness; anxiety; and increase in worry.

In a given individual we have found that the responses frequently occurred in unpredictable combinations; for example, an annoying restless

irritability was often encountered. In general, mental, speech and motor activity is more affected than elevation of mood. There is no preliminary indication as to the type of reaction that will obtain in a given person. In some patients the unpleasant effects appeared to lessen after a few days of medication; in others these seemed to accumulate. Tolerance to the drug is established in some individuals so that the same dose has little effect after a time. We have observed the formation of a habit in certain persons, particularly in neurotic types who crave medication.[4] Individuals with strong alcoholic habits especially relish the drug. The physiological effects appeared to predominate over the psychological in some instances.

These observations on the psychological effects produced by benzedrine sulfate in normal persons and in patients have been confirmed by numerous investigators.[5]

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[4] *Op. cit., Psychiat. Quar.*, 13, 1939, (in press); *N. Y. State Med. J.*, (in press).

[5] M. Prinzmetal and W. Bloomberg, The use of benzedrine for the treatment of narcolepsy, *J. Amer. Med Assn.*, 105, 1935, 2051-2054.

S. A. Peoples and E. Guttmann, Hypertension produced by benzedrine: Its psychological accompaniments, *Lancet*, 1, 1936, 1107-1109.

A. Myerson and M. Ritvo, Benzedrine sulfate and its value in spasm of the gastro-intestinal tract, *J. Amer. Med. Assn.*, 107, 1936, 24-26.

E. Guttmann, The effect of benzedrine on depressive states, *J. Ment. Sci.*, 82, 1936, 618-620.

A. Myerson, Effect of benzedrine on mood and fatigue in normal and neurotic persons, *Arch. Neurol. & Psychiat.*, 36, 1936, 816-822.

J. H. Fisher, Cardiovascular effects of benzedrine sulfate, *Lancet*, 1, 1937, 52.

D. M. O'Connor, Benzedrine, *Brit. Med. J.*, 1, 1937, 43.

M. H. Nathanson, The central action of beta-aminopropylbenzene (benzedrine), *J. Amer. Med. Assn.*, 108, 1937, 528-531.

M. J. Shapiro, Benzedrine in the treatment of narcolepsy, *Minn. Med.*, 20, 1937, 28-31.

D. L. Wilbur, A. R. MacLean, and E. V. Allen, Clinical observations on the effect of benzedrine sulfate, *Prof. Staff Meetings Mayo Clinic*, 12, 1937, 97-104; Clinical observations on the effect of benzedrine sulfate: A study of patients with states of chronic exhaustion, depression, and psychoneurosis, *J. Amer. Med. Assn.*, 109, 1937, 549-554.

P. J. Reiter, Erfaringer on benzedrin (Foreløbig Meddelese), *Ugeskr. f. Laeger*, 99, 1937, 459.

H. Storz and R. Kirk, über eine neue blutdricksteigernde Substanz, *Deut. Med. Wochnsch.*, 63, 1937, 393.

J. Delmond and R. Vercier, Benzedrine as a stimulant in nervous fatigue and vegitative disorders, *Ann. méd.-psychol.*, 95, 1937, 323-328.

E. Guttmann, and W. Sargent, Observations on benzedrine, *Brit. Med. J.*, 1, 1937, 1013-1015.

P. Solomon, R. S. Mitchell, and M. Prinzmetal, The use of benzedrine sulfate in postencephalitic Parkinson's disease, *J. Amer. Med. Assn.*, 108, 1937, 1765-1770.

C. Bradley, The behavior of children receiving benzedrine, *Amer. J. Psychiat.*, 94, 1937, 577-585.

H. Ulrich, Narcolepsy: Its treatment with benzedrine sulfate, *New Eng. J. Med.*, 217, 1937, 696-701.

P. G. Schube, M. C. McNanamy, C. E. Trapp, and A. Myerson, The effect of benzedrine sulfate on certain abnormal mental states, *Amer. J. Psychiat.*, 94, 1937, 27-32.

In this connection the data accumulated by Gwynn and Yater on the effect of the drug in 147 students is worth citing.[6] The students analyzed their own reactions and concluded that the chief effect of the medication was as follows: psychic stimulation, 71; insomnia, 28; nervousness, 13; with palpitation, headaches, vertigo, dryness of the throat, fatigue, depression, euphoria and epigastric pain as the outstanding response reported by the other subjects. Only two observed no effect. In answer to specific questions the following results were obtained in the same group of students: 126 suffered from insomnia, 113 felt 'peppy,' 81 complained of nervousness, 72 became exhilarated, 61 noticed increased power of concentration, 42 were more talkative, 19 had headaches, 8 observed decreased power of concentration, and a small number recorded vertigo, depression, uneasiness, fullness or throbbing in the head, and fatigue. Schilder, after utilizing the drug during psychoanalysis in further confirmation, states "the results are seemingly due not alone to the structure of the drug but also to the structure of the personality."[7]

The sleep-combatting action of the drug has been clearly demonstrated by the disappearance or amelioration of sleep attacks in patients with narcolepsy.[8] Molitch and Poliakoff utilized this same action in overcoming nocturnal enuresis in juvenile patients who had previously failed to respond to the call to urination because of excessively deep slumber.[9] In previous publications we have demonstrated that benzedrine sulfate produces sufficient stimulation of the central nervous system to arouse patients comatose from mild doses of barbiturates and from alcohol.[10]

Psychometric tests given before and after benzedrine sulfate administration have furnished additional evidence of the stimulating action of the drug on the central nervous system. People and Guttmann found improved performance on the con-

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D. E. Donley, Observation on the use of benzedrine in the psychoses, *Ohio State Med. J.*, 33, 1937, 1229-1232.

J. Hill, Benzedrine in sea-sickness, *Brit. Med. J.*, 2, 1937, 1109-1112.

A. M. Meerloo, Benzedrine sulfate as a brain stimulant, *Nederl. Tijdsch. v. Geneesk.* (Amsterdam), 81, 1937, 5797-5800.

D. J. Sobin, Stramonium plus benzedrine for the relief of postencephalitic sequelae, *J. Speech Disorders*, 2, 1937, 205-206.

L. F. Woolley, The clinical effects of benzedrine sulfate in mental patients with retarded activity, *Psychiat. Quar.*, 12, 1938, 66-83.

P. L. Davis and W. B. Stewart, The use of benzedrine sulfate in postencephalitic Parkinsonism, *J. Amer. Med. Assn.*, 110, 1938, 1890-1892.

[6] H. B. Gwynn and W. M. Yater, A study of temporary use of benzedrine sulfate in 147 supposedly normal young men, *Med. Ann. D. C.*, 6, 1937, 356-359.

[7] P. Schilder, The psychological effect of benzedrine sulfate, *J. Nerv. & Ment. Dis.*, 87, 1938, 584-587.

[8] Prinzmetal and Bloomberg, *op. cit.;* H. Ulrich, C. E. Trapp, and B. Vidgoff, The treatment of narcolepsy with benzedrine sulfate, *Ann. Int. Med.*, 9, 1936, 1213-1221; Davidoff, *op. cit.;* Guttmann and Sargent, *op. cit.;* Nathanson, *op. cit.;* Shapiro, *op. cit.;* W. Bloomberg, The use of benzedrine in postencephalitic Parkinsonism (discussion), *J. Nerv. & Ment. Dis.*, 85, 1937, 202; H. N. Tihen, Treatment of narcolepsy with benzedrine sulfate, *J. Kansas Med. Soc.*, 38, 1937, 208; Ulrich, *op. cit.;* D. Young and W. B. Scoville, Paranoid psychosis in narcolepsy and possible danger of benzedrine treatment, *Med. Clin. N. Amer.*, 22, 1938, 637-647.

[9] M. Molitch and S. Poliakoff, The effect of benzedrine sulfate on enuresis, *Arch. Ped.*, 54, 1937, 499-501.

[10] *Op. cit., Proc. Soc. Exper. Biol. & Med.*, 38, 1938, 181-184; *Psychiat. Quar.*, 13, 1939, (in press).

tinuous addition test.[11] Sargent and Blackburn reported that 46 mentally disordered patients showed an average increase of 8% in the response to Cattel's Intelligence tests.[12] Molitch and Sullivan found the performance of 50 boys on the New Stanford Achievement Test improved in 96%.[13] Molitch and Eccles[14] found improved performance in 93 boys in tests of the strength of grip (Smedley dynameter), motor coördination (Witmer form board test), memory for design (reproduction of card designs), and one of three highly standardized intelligence tests (Kent E.G.Y., Morgan, and Kuhlman-Anderson). Bradley observed improvement in school performance in half of 30 behaviour problem children.[15] We have noted more elaboration of detail and more coördination of activity in the Rorschach test in two cases.[16] McNamara and Miller were unable to demonstrate better performance in arithmetic problems;[17] their period of study was not, however, sufficiently prolonged, and the observations were made at too long an interval after the administration of the drug. Barmack,[18] with a similar study of arithmetic problems, demonstrated that benzedrine sulfate increased the amount but not the efficiency of mental work performed, and prevented the development of boredom, strain, irritation, fatigue, sleepiness, and inactiveness in continuous, repetitive work. He concluded that the effect of benzedrine was principally on the inclination rather than on the ability to perform such tasks.

In our experience, persons with inherently low mental levels showed little alteration in intelligence scores after benzedrine sulfate, although patients with early toxic or infectious psychoses were apt to increase their rating as the abnormal mental state cleared. It seems likely that the general efficiency rather than the specific ability is influenced by the drug in most cases.

Relatively few studies of the effect of benzedrine sulfate on the central nervous system have been made on animals. Alles,[19] and Detrick et al.[20] observed that benzedrine sulfate tended to awaken animals from anesthesia with barbiturates. Ehrich and Krumbhaar reported that the drug subcutaneously produced excitement in albino rats.[21] The animals began to run around excessively, gnaw their cages and

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[11] Peoples and Guttmann, *op. cit.*

[12] W. Sargent and J. M. Blackburn, The effect of benzedrine sulfate on intelligence scores, *Lancet*, 2, 1936, 1385-1387.

[13] M. Molitch and J. P. Sullivan, The effect of benzedrine sulfate on children taking the new Stanford achievement test, *Amer. J. Orthopsychiat.*, 7, 1937, 519-522.

[14] M. Molitch and A. K. Eccles, The effect of benzedrine sulfate on the intelligence scores of children, *Amer. J. Psychiat.*, 94, 1937, 587-590.

[15] Bradley, *op. cit.*

[16] Davidoff and Reifenstein, *op. cit.*, *Amer. J. Psychiat.*, (in press).

[17] W. J. McNamara and R. E. Miller, The effect of benzedrine sulfate on mental work, *Psychol. Rec.*, 1, 1937, 78-84.

[18] J. E. Barmack, The effect of benzedrine sulfate (benzyl methyl carbinamine) upon the report of boredom and other factors, *J. Psychol.*, 5, 1938, 125-133.

[19] G. A. Alles, The comparative psychologic actions of dl-B-phenylisopropylamines: I. Pressor effect and tozicity, *J. Pharmacol. & Exper. Therap.*, 47, 1933, 339-354.

[20] L. E. Detrick, R. Millikan, F. S. Modern, and C. H. Thienes, On the pharmacology of phenylisopropylamine (Benzedrine), *ibid.*, 60, 1937, 56-68.

[21] W. E. Ehrich and F. B. Krumbhaar, The effect of large doses of benzedrine

splash in their dishes of water. Some showed very queer movements, ran backwards or in circles, had difficulty walking, often sat on their hind legs pawing the air and falling over backwards. All of the animals fought a great deal more than the control rats. Two were found gnawing their own thoracic skin. Skinner and Heron stated that benzedrine sulfate would restore to full efficiency a conditioned reflex which had been allowed to become partially extinguished by a number of days of non-performance.[32] That the drug would cause rats to produce a greater output of work was clearly demonstrated by Zieve.[33] His animals were confined in revolving activity drums, the revolutions of which were counted before, during and after the drug had been administered. Approximately twice as much work was performed by the animals under the influence of the drug. Wentink reported that benzedrine sulfate increased the rate of response of rats to conditioned reflexes 142%.[34]

Although none of our subjects were aware of the nature of the medication or of the symptoms produced by it, the psychic effect of the administration itself cannot be disregarded entirely. The following are illustrative cases.

Case 1 (Normal). Four hours after the first medication the subject became very active. He telephoned for flowers to be sent to a sick friend, but shortly after, becoming impatient, he hurried several miles to the florist shop and delivered the gift in person. He said "I wanted action, and I got it!" Later he complained of fatigue. He slept well that night. The second day he noticed a general feeling of depression. The next day he was restless, "felt like reeling" and was mentally dull and retarded. On the fourth day he "felt terrible and groggy." On the next day he was "pepped up" and mentally alert. The following day he was annoyed by trivial errors in his work. Throughout the remainder of the period he continued to sleep poorly, made many of the same complaints and toward the end became forgetful.

Case 2 (Normal). Four hours after medication on the first day the patient complained of a 'nervous' feeling in the stomach, followed by abdominal pain and nausea. Two hours later she felt better, was more active and ambitious, but was restless. She slept only three hours. On the next day shortly after the medication she became more active but was not able to accomplish much and described herself as 'flighty.' She stated that trivial matters appeared exaggerated to her. That evening she was fatigued, and during the night again slept poorly. On the third day she found it an effort to work, complained of restlessness, exhaustion and excessive fatigue. In the evening, however, she was more active and cheerful. On the following day she noticed marked tremor of the hands; later she was active and had a desire to complete her work, but noticed difficulty in connecting her words. In the evening although she felt much fatigued, she was not sleepy and continued active until 4 A.M. The next morning she complained of vertigo, partial syncope, and

sulfate on the albino rat: Functional and tissues changes, *Ann. Int. Med.*, 10, 1937, 1874-1888.

[32] B. F. Skinner and W. T. Heron, Effect of caffeine and benzedrine upon conditioning and extinction, *Psychol. Rec.*, 1, 1937, 340-346.

[33] L. Zieve, Effect of benzedrine on activity, *ibid.*, 1, 1937, 393-396.

[34] E. A. Wentink, The effects of certain drugs and hormones upon conditioning, *J. Exper. Psychol.*, 22, 1938, 151-163.

extreme anxiety. She became very fearful and at her request the medication was discontinued. She continued to complain of excessive fatigue for several days thereafter.

Case 3 (Manic depressive, depressed). Prior to medication the patient was depressed, physically retarded, underproductive, hopeless in his outlook and lacking in ambition. He became talkative within an hour after taking the drug. Three hours later he was "feeling much better" but complained of fatigue. He did not sleep well. The second day he appeared depressed three hours after taking the drug, but from six to eight hours later he stated that he felt "good" and he slept well that night. On the fourth day he remarked that he was very happy, cheerful and alert. On the sixth day he was jovial. By the tenth day he was singing; he stated that he felt "great" and appeared to have reached approximately his normal state. He was quite active. On the twelfth day he was depressed shortly after taking the drug but toward evening appeared to be in better spirits. On the fourteenth day he was very talkative but complained of depression. Throughout the period he worked more efficiently in the ward. In general his mood appeared less affected than his speech response, motor activity and general efficiency.

Case 4 (Dementia precox, catatonic type). Prior to medication the patient was markedly underproductive, self-absorbed, very retarded, inactive, untidy, apathetic, dull, listless, timid, and passively resistive. She refused to move or to do anything and had to be spoon-fed. She experienced delusions, and auditory hallucinations.

Two hours after receiving the drug on the first day the patient began to talk and told her husband that she felt uncomfortable. The next day she became more resistive and uncoöperative, but worked a puzzle, smiled occasionally and stated that she felt better. That night she was unable to sleep, became fearful and restless, and finally much agitated, and begged to be taken home. The following day she began to eat spontaneously, assisted willingly with the ward work, and became more talkative and coöperative. She recognized her hallucinations as 'queer' ideas, which she stated were disappearing. She was more alert, more appreciative of her surroundings and smiled spontaneously, but continued fearful at times. On the morning of the seventh day of medication the patient complained of feeling worse, was tearful, more uncoöperative and fearful but assisted in the hospital work. On the following day she was resistive, agitated and uncoöperative prior to medication, but afterward became cheerful and talkative. In the evening she was seclusive and was unable to sleep. After two weeks the patient assisted with the ward work, and applied herself well in occupational therapy, but was able to sleep only three hours at night and continued irritable, confused, and fearful of impending disaster. After the 25th day of medication the patient began to show definite improvement, took interest in her personal appearance, showed more purposeful activity. She continued to become more coördinate in her activities, became interested in her surroundings and began to ask questions, was cheerful, friendly and talked freely about herself. She remarked that she felt like a different person.

Case 5 (Alcoholic psychosis, delirium tremens). Prior to medication the patient was confused, delirious, disoriented, overproductive, complained of visual hallucina-

tions of snakes and worms four to eight feet long which he was pulling from his mouth, and of elephants which he saw on the wall; he then lapsed into stupor. After a dose of benzedrine sulfate intravenously he aroused from the stupor and began again to talk of the snakes. Following another dose of the drug he suddenly remarked that the snakes had disappeared, became better oriented, much less confused, but very restless. In several hours he was cheerful and happy, stated that he felt very well, was facetious, talkative and spoke relevantly and coherently of the events prior to his admission. The next day he assisted in the ward work, attended occupational therapy, occupied himself in reading magazines and listening to the radio. He was discharged to his home ten days after admission, completely recovered.

Case 6 (Normal). Within six hours the subject noted increased activity and alertness. He called his superior officer on the telephone, insisted on taking an examination for which he was not eligible and became quite agitated during the discussion. He was embarrassed about the incident a few hours later. On the third day he noted exaggerated and prolonged physical and mental alertness. On the fourth day general sluggishness and malaise. On the seventh day he was depressed. After the eighth day he observed, in addition, increased physical and mental activity; feeling of warmth; and decreased requirement of sleep. After the drug was discontinued he felt depressed and fatigued for several days.

Case 7 (Traumatic psychosis, post-traumatic personality disorder). Prior to medication the patient was confused, depressed, dull and apathetic, retarded, quiet, rather uncommunicative, but talked about his accident and experienced auditory hallucinations. On the first day of medication he was worried, restless, anxious and complaining. On the the next day he was fatigued and weak, but much more talkative and friendly with marked improvement in recent and remote memory. At times he appeared moderately depressed, and seclusive, stating that he did not feel like living. The following day he was extremely talkative, more active, and willing to assist in the ward work. His IQ rating was found to have risen from 68 to 79. The next day he felt much better, laughed, told his story freely, and was cheerful and coöperative. He continued to improve during the rest of his stay in the hospital.

Case 8 (Psychoneurosis, hysteria). Prior to medication the patient was quiet, depressed, anxious and tearful, with retarded speech. He was harrowed by obsessional memories of war experiences and marital difficulties, and had numerous somatic complaints. Within four hours he became perturbed and agitated. He appeared to have a marked reaction and voluntarily went to bed. Motor activity was much diminished and he fell asleep for about an hour. The next morning, within three hours he complained of fatigue. He again experienced a reaction and voluntarily went to sleep. Several hours later, however, he felt much better and became very talkative. He experienced the same symptoms on the third and fourth days but became more cheerful. The fifth day he appeared delirious for a time and complained of "water ebbing up and down his body to his neck." These sensations soon disappeared. The next day he could not remember anything that had happened and was confused. On the seventh day he was improved and became jovial and talkative.

although the motor activity increased only slightly, and there appeared to be no change in his general efficiency. Toward the end he was occasionally drowsy.

Case 9 (Depressed alcoholic). Prior to medication the patient was depressed, physically retarded, dull and underproductive. One hour after taking the drug he felt considerable motor and mental acceleration and stated that he felt "good." There was a marked increase in talkativeness. At the end of two hours he was confused and unsteady on his feet but insisted that he was feeling "very strong." After three hours he was restless, talkative, whistled constantly, appeared very cheerful and for the first time since his admission he asked to go on the porch. He continued to be cheerful and to pace about for the rest of the day.

Case 10 (Dementia precox, paranoid type). Prior to medication the patient was quiet, seclusive, and depressed. He had attempted suicide with phenobarbital. A few hours after taking benzedrine sulfate he became more active, and cheerful. On the second day he was irritable and depressed, seclusive, with diminished physical activity. Later he was restless and unable to sleep. On the third day he was more talkative, and more active, walking up and down the hall. However, he became fearful and complained of anorexia. On the next day he was much more cheerful and coöperative but stated that he felt 'nervous.' On the fifth day he expressed regret for a previous outburst of temper, showed considerable physical activity, laughed and talked considerably, began to sing to himself. He slept well that night. The next morning he complained of feeling 'terrible' and had nausea, cold hands and excessive restlessness. The following day he was better again, and was cheerful and coöperative. During the rest of the period of administration he continued to have variable reactions, at times cheerful, coöperative and physically active; at other times restless and irritable. At this time he uncovered in considerable detail his paranoid reactions toward his family, his physicians, and the hospital, all of which he had previously withheld.

Summary

Benzedrine sulfate stimulates the central nervous system, producing a variety of psychologic reactions which are unpredictable and at times paradoxic. A description of these reactions in 200 normal persons and mentally deranged patients is herein presented, with illustrative cases.

The stimulating effect of the drug has been demonstrated clinically in the amelioration of narcoleptic sleep attacks, in the dispelling of coma due to barbiturates and to alcohol, and in the increased performance on psychometric tests. Limited confirmatory data has been obtained from a few animal experiments. The need for further controlled objective psychologic investigation of the action of benzedrine sulfate is indicated.

FORGETTING OF MEANINGFUL MATERIAL DURING SLEEP AND WAKING

By EDWIN B. NEWMAN, Swarthmore College

The marked difference between the forgetting curves obtained during intervals of sleep and waking argues very strongly against the conventional theory that accounts for forgetting in terms of the law of disuse.[1] This difference suggests rather that the process of forgetting is an active process influenced by activities interpolated between original learning and testing. If this conclusion is accepted, and it has been demonstrated conclusively by a number of recent writers,[2] the comparison of forgetting curves obtained during sleep and during waking should give us an instrument with which to study the effectiveness of various factors essential to retention.

Using meaningful material in some preliminary experiments, we attempted to obtain results similar to those of Jenkins and Dallenbach and of Van Ormer. Short stories were read by the observers and reproduced after equal intervals of sleep and waking. No reliable difference was found between the two conditions, a result which we were inclined to attribute to faulty technique. Control experiments and more careful analysis of the results, however, showed that our technique was not faulty, and that a consistent and reliable difference between meaningful and nonsense material was to be expected. To make this comparison more clear, the following formal experiments were performed.

EXPERIMENTAL PROCEDURE

The material used consisted of three short stories, each about three hundred words in length and constructed so that it contained a number of items which were not essential to the plot. After some experimentation, the material was so arranged that each story contained an approximately equal number of essential and non-

* Accepted for publication May 29, 1938.

[1] This study is the result of work done in undergraduate honors seminars at Swarthmore College. Credit is due to Louise Watkins for the original distinction between essential and non-essential material, to Deborah Wing for the final form of the stories used, and to Marion Rous and Margaret Chase for a major part of the experimentation and the final scoring.

[2] The earlier work on sleep and forgetting has been adequately summarized by E. B. Van Ormer, (Retention after intervals of sleep and of waking, *Arch. Psychol.*, 21, 1932, (no. 137), 1-49) and will not be repeated here. Cf. also, R. Heine, Ueber Wiederkennen und rückwirkende Hemmung, *Zsch. f. Psychol.*, 68, 1914, 161-236; and J. G. Jenkins and K. M. Dallenbach, Obliviscence during sleep and waking, this JOURNAL, 35, 1924, 605-612. The theory that forgetting is a function of retroactive inhibition has been clearly stated in J. A. McGeoch, Forgetting and the law of disuse, *Psychol. Rev.*, 39, 1932, 352-370, and in subsequent papers by the same writer.

essential points. The stories are reproduced below. The essential material is printed in italics, and the twelve parenthetical numbers mark the twelve essential points used in scoring. The non-essential material was divided correspondingly into twelve non-essential points.

THE LONG EXILE (L)

One beautiful day in the middle of summer *a young farmer named Ivan started out from his farm for the fair* (1) singing gaily, for he had many vegetables to sell. *On the way he met a merchant and they journeyed together, stopping at the same hotel for the night* (2). The next morning Ivan started off early while the sun was just rising, but as he was riding happily on his way, *he was stopped by some soldiers who questioned him carefully as to his whereabouts the night before* (3). *In searching his belongings they found a blood-stained knife in his bag* (4). *To Ivan's frantic questioning, they finally told him that the merchant with whom he had spent the night had been found murdered in his bed* (5). By questioning all who might know about the case, *the police had concluded that, since Ivan was the only one that had been with the merchant, the guilt pointed to him* (6). *With this evidence he was exiled for life* (7), never again to see his family and friends or the beautiful fields of his own town. Many long years he was in prison, gradually growing older and grayer while the best part of his life slipped away from him slowly. *One day a new group of convicts arrived to pay the penalty for their crimes* (8). There were a great many of them—old and crippled, young and in good health. In talking to them as was his custom with a new group, *Ivan chanced upon the startling and amazing discovery that one of these convicts was the real murderer of the merchant, for whose death he, Ivan, was paying the penalty* (9). On this discovery his whole soul was torn in a great struggle; *here was his chance to be set free* (10), to see his homeland again; yet with a sickening heart he realized what it would mean to the other convict, dooming him to life imprisonment in place of his original short sentence. The convict had just left his wife and children who were sick from the famine and needed his care. He hoped to return soon, whereas there was no one that needed Ivan or would be there to welcome him after so many years. With calmness and deliberation he kept his secret to himself and worked steadily in the mines, growing feebler and feebler. *After some time the real murderer, weighed down by his own conscience, confessed his guilt* (11). *On the day they came to release Ivan,* a day in the middle of spring, that was sunny and warm, *they found that he was dead* (12).

THE PERFECT ALIBI (P)

One day a group of three army intelligence officers walked into the office of detective Jim Dudley, and asked his assistance. *A sergeant at Camp Dix had disappeared three months before* (1). During that time they had tried to locate him, but with no success. *They had just now discovered his body in a clump of bushes, and it was clear that he had been murdered* (2). Dudley arrived on the scene with a large staff of detectives that started to work. *They soon established the fact that the victim must have been slain by someone in his own company* (3). However, no one was found who had any particular motive for killing him, but it was established that he had been a small-scale loan shark. *The next step was to question each man in the company as to his actions on the day of the sergeant's disappearance* (4). A stenographic report was made of each man's story to be used for future reference. *In examining the stories, those men who were unable to tell anything about their activities on the day were immediately dismissed from consideration* (5). Naturally, the news was received with many sighs of relief. *However, there were ten who could give some account of their activities on that day, and these men were questioned for further details* (6). The preliminary reports hadn't furnished any definite clues to the murderer. *This time nine became hopelessly involved in contradictions and they were eliminated* (7). Since Dudley's work was carried on secretly, no one yet knew clearly the purpose behind it all. *He discovered that the tenth man was still sure of his actions, not only on the day of the murder, but the day after and*

before (8). When questioned he showed no signs of nervousness or guilt. *But Dudley knew he had his murderer* (9), *for nobody who was not repeating a story he had made up and memorized could remember that much about his action three months before* (10). If anyone should try it they'd find it almost impossible to remember the barest outline. *Having spotted his man in this way, Dudley found that the sergeant had lent money to the prisoner who was finding it hard to repay* (11). *He then had little difficulty in forcing the prisoner to confess that to wipe out the debt he had wiped out the sergeant* (12). The confession was carefully recorded and signed by the prisoner to prevent any repudiation.

The Work of Art (W)

One cold day in January, *Sasha burst into the doctor's consulting room carrying a big package under his arm,* carefully wrapped up in newspaper and tied with a piece of heavy cord. *When the doctor came out, he handed him the bundle* (1), patting it gently as though it were very precious, and *sputtered out, "Mama says to thank you for saving my life. We want you to have this present as a token of our gratitude"* (2). *He explained that it had been left in his father's antique shop when he died and they had kept it as a souvenir ever since then* (3). "Mama and I keep on my father's business now," he added. Sasha unwrapped the present for the doctor, taking the paper away carefully as though wishing to create suspense. *What was revealed was a candelabra* (4) of old bronze and artistic workmanship. *On the pedestal stood two female figures in the costume of Eve and in attitudes for the description of which one would have neither the courage nor the fitting temperament* (5). The doctor, taking in the figures, spoke in a non-committal tone to the effect that it was a fine piece of work well done; *but he tried to refuse the gift, saying that it wasn't quite proper for his consulting-room* (6). *Sasha not only wouldn't listen to his refusal, but he said sadly that his only regret was that he didn't have the other member of the pair* (7). He spoke in reverent tones of art that should be appreciated for its own sake. *When Sasha went away, the doctor, still having the gift, determined to give it to a bachelor-friend of his who might appreciate it* (8). The bachelor accepted the gift, but concerned with his own reputation, *decided he'd better pass it on to a comedian that he knew, who might have more use for such a piece of art* (9). He took it to the stage dressing-room where the present was greatly admired by all his friends. *The comedian realized he couldn't keep it, and when his hairdresser, who always had good ideas, suggested selling it to an antique shop owned by a woman and her son, he carried out her suggestion immediately* (10). After two days, when the doctor had completely forgotten the episode, *Sasha burst into his room with a big bundle under his arm, crying, "Doctor, happily for you we have succeeded in picking up the pair to your candelabra* (11). Mama is so happy that we can make our present complete," and he set down the candelabra before the doctor. *The doctor tried to say something, but said nothing. He could not speak* (12).

Eleven *S*s, all college students, took part in the final experiment. They read each story at a different time of day and reproduced it approximately 8 hours after reading. The order of the stories and the relation between the story learned and the time of learning was rotated for the various *S*s, and the order for each *S* is indicated by the letters in Table I. Comparisons were not made at other than the 8-hr. interval because we expected to find the maximum difference between sleep and waking with this interval. The scoring of the stories was done by three independent judges in order to obtain as great objectivity as was possible. Each judge was given a list of the 12 essential and 12 non-essential points and was asked to compare the reproduction with the original story in terms of these points. It was found that the judges were in substantial agreement about the amount of material reproduced, even though they disagreed in some instances about individual points. The percentages of Table I are means of the ratings of the three judges.

Results

The results of the experiment are presented in Table I, in which every percentage in the body of the table represents the proportion of the items recalled by one *S* in his reproduction of one story. In the last line are the median results for each kind of material at the different times of day. These show first that the essential material is always reproduced much better than the non-essential material: the averages are 86% and 30% respectively. However, the *essential* material is reproduced equally well at the three different times of day. Eighty-seven percentage of the material was reproduced after 8 hr. of sleep, while 84% and 87% were reproduced after 8 hr.

TABLE I

Percentage of Material Reproduced by Each *S* after an Interval of Eight Hours

Morning and Afternoon = reproduction after waking activities. Night = reproduction after sleep. Every percentage is the mean of ratings by three judges. L, P and W indicate the story which was used as material.

S	Essential material			Non-essential material		
	Morning	Afternoon	Night	Morning	Afternoon	Night
A	P 68	L 96	W 74	P 19	L 46	W 47
B	W 84	P 81	L 92	W 25	P 19	L 33
C	L 93	W 74	P 85	L 18	W 10	P 32
D	L 95	W 88	P 78	L 41	W 17	P 48
E	P 74	L 94	W 85	P 35	L 19	W 36
F	W 93	P 83	L 96	W 23	P 18	L 40
G	W 70	L 87	P 78	W 25	L 33	P 48
H	L 71	P 84	W 87	L 27	P 15	W 47
I	W 70	P 72	L 92	W 0	P 11	L 29
J	L 92	W 99	P 93	L 64	W 86	P 72
K	P 93	L 95	W 100	P 48	L 65	W 80
Median	84	87	87	25	19	47

of waking activities during the morning and afternoon respectively—a result which confirms our preliminary findings. On the other hand, the *non-essential* material shows a different picture. After 8 hr. of sleep, 47% of the material was reproduced, while but 25% and 19% was reproduced after the two waking intervals. Almost exactly half as much material was reproduced after the waking intervals as was retained after sleep. The non-essential material behaves, therefore, in very nearly the same manner as did the nonsense syllables of previous investigators.

Detailed statistical comparisons are scarcely justified with such a limited number of cases. The scores compared for each individual *S* show, however, the same result as do the scores for the group. With the non-essential material, 10 of the 11 *S*s show a clear-cut superiority for the sleeping interval over the average of the day intervals. With the essential material, 7 *S*s show some superiority for the sleeping interval, while 4 reproduced

more after the waking interval. *J* represents an exception to the general trend, largely because she reproduced all the stories with almost verbatim exactness.

Discussion

The results of the present experiment lend support to the contention that forgetting is essentially a matter of interference—proactive or retroactive inhibition. There is a clear-cut parallel between forgetting and inhibition in the results for the non-essential material; this material which is much less well remembered when compared with the essential material is also the material which shows the marked gain as a result of interpolating sleep rather than waking. Forgetting is associated with inhibition in this case. Up to this point the findings and interpretations are in agreement with those of Heine, Jenkins and Dallenbach, and Van Ormer; but now we can go one step further, showing that meaningfulness of the material is an important condition of retroactive inhibition. Material as meaningful as the stories used in these experiments has inherent in it some factor which effectively prevents retroactive inhibition.

This conclusion is not particularly surprising. It is implicit in the relationship which has been assumed to hold between forgetting and retroaction when we recall how little forgetting usually occurs with meaningful material. McGeoch and McKinney found relatively small amounts of retroactive inhibition when using poetry and prose as their learning materials.[3] On the other hand, other studies have pointed to quite the opposite conclusion. McGeoch and McDonald showed that the inhibitory effect of interpolated material could be increased by making each item in the interpolated list a synonym of the corresponding item in the original list, and that this inhibition was the stronger, the more perfectly were the two words synonymous.[4] That this effect was the result of the meaningfulness of the words and not the result of serial position was clearly shown by McGeoch and McGeoch.[5] Thus, in these experiments the increase of mean-

[3] J. A. McGeoch and F. McKinney, Retroactive inhibition in the learning of poetry, this JOURNAL, 46, 1934, 19-33; The susceptibility of prose to retroactive inhibition, *ibid.*, 429-436. They state: "To ask why poetry should suffer so small a loss from retroactive inhibition is tantamount to asking why meaningful material is forgotten less rapidly than non-meaningful, an inquiry which points beyond the intent of our discussion" (p. 28). They suggest, however, that "it may be that relative degrees of organization [*sic!*] of the original and interpolated materials are very important in determining amount of retroactive inhibition" (p. 31 f.).

[4] J. A. McGeoch and W. T. McDonald, Meaningful relation and retroactive inhibition, this JOURNAL, 43, 1931, 579-588.

[5] J. A. McGeoch and G. O. McGeoch, Studies in retroactive inhibition: VI. The influence of the relative serial positions of interpolated synonyms, *J. Exper. Psychol.*, 19, 1936, 1-23.

ingfulness of the material increased rather than decreased interference.

This ambiguity in prediction that is introduced by the meaningfulness of the material requires some hypothesis about the nature of meaningfulness and the way in which it operates to aid or hinder learning. A theory of meaning in terms of associated context, the items of which are connected by indifferent bonds to the verbal core, has difficulty in explaining these results. If a mere increase in the number of bonds between the verbal core and other items outside the list to be learned can be so effective in producing inhibition (or negative transfer) in the case of the synonyms, why should it not be equally effective in the case of the prose and poetry? It must be clear that some concept other than that of associations, bonds, identical elements or the like must be supplied to differentiate between the two sets of results. Such a concept would require that meaning reside in the structure or organization of the material to be learned, in the organized 'sense' of the story, in a 'context' of meaning which does not consist of indifferent bonds. This view would hold that meaning *may* consist in part of 'contextual' material, but it would add two things. The first is that the relations of core to context are integral with the material, consisting of such relations as equivalence, inclusion, superordination, subordination, centeredness, and terminal, intermediate, progressive or symmetrical character. The second is that the *totality* of these relations frequently, if not invariably, forms a structure which has whole-properties of its own. The neatness of the solution to a problem, the incompleteness of a continued story are limited illustrations of such whole-properties.

The case for this concept of meaning is particularly strong in the present experiment. It cannot be argued that the difference between the more meaningful and less meaningful material is to be found in the number of previous associations with the individual units of sensible or nonsense material. No one could reasonably hold that the particular words which form the non-essential items had numbers of connections different from the words comprising the essential items. It must be clear that the structure of the story as a whole and the relation of the individual items to this structure are the determining factors which account for the differences found in this experiment.

Conversely, this concept is able to explain the interference in the case of the synonyms as interpolated material. The series of adjectives forms at best a sequence in which the relation of each item to the succeeding one is extremely peripheral in the meaningful structure of each word. On the other hand, there is a relation between the synonyms in the two lists which is not only strong, but which has the additional property of being central

to the meaningful structure, and also the even more important property of the symmetrical relation of equivalence. Finally, there is in the list as a whole no structure which may serve as a framework for the individual adjectives. The sequence of adjectives is, in our sense, meaningless.

An excellent example of the operation of just such factors as those mentioned above is to be found in the experiments of Köhler and Von Restorff in which it was shown that interference or inhibition is produced, even within a single series, by the massing of equivalent, homogeneous material.[6] This interference is reduced when the material is heterogeneous, but a much larger reduction is obtained when the series has at least a minimal amount of organized character—in their experiments, a figure-ground character where one heterogeneous element stands out on a homogeneous ground. Correspondingly, the present results show that the organization furnished by the meaningful plot of the story is effective in preventing that state of crowding of unrelated items which in other situations is the essential condition for interference and for forgetting.

SUMMARY

(1) Ideas essential to the plot of a story are reproduced equally well after intervals of sleep and waking (87% compared with 86%), while non-essential items are reproduced twice as well after an interval of sleep as after an interval of waking (47% compared with 23%).

(2) This result argues that the organized character of the material is a condition significantly affecting retroactive inhibition and forgetting.

[6] H. von Restorff, Ueber die Wirkung von Bereichsbildungen im Spurenfeld, *Psychol. Forsch.*, 18, 1933, 299-342, esp. 313-328.

OXYGEN-TENSION AND 'THE HIGHER MENTAL PROCESSES'

By MADISON BENTLEY, Library of Congress

Recent physiological studies by Ernest Gellhorn and associates bear upon the effect on the central nervous system of reducing oxygen in the respiratory air. These studies fall within a large experimental literature of the last two decades, a literature which Gellhorn found to be defective on the side of sensory damage done by low oxygen-tension.[1] Once embarked upon the quest for local damage, however, this physiologist was carried from the senses to the brain and from the brain to that indefinite and dangerous region of 'the higher mental processes.' As regards central effects of low alveolar oxygen, this author came to believe that "such divers functions of the central nervous system as visual intensity discrimination, the latent period and intensity of negative after-images, auditory acuity, muscular coördination, associations and simple mental tasks . . . are impaired under approximately the same degree of oxygen deficiency."[2]

As to the effect upon the 'simpler sensory functions' of stimulus, differential limen, and time-interval between photic excitation of the eye and certain after-effects, little need be said in this context beyond the observation that the Gellhorn experiments have neglected many checks, refinements, and methodical precautions which careful psychologists have found to be necessary in such determinations.[3]

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[1] E. Gellhorn and I. Spiesman, Influence of variations of O_2 and CO_2 tension in inspired air upon hearing, *Proc. Soc. Exper. Biol. & Med.*, 32, 1934, 46-47.

[2] E. Gellhorn, The influence of carbon dioxide in combatting the effect of oxygen deficiency on psychic processes, with remarks on the fundamental relationship between psychic and physiologic reactions, *Amer. J. Psychiat.*, 93, 1937, 1413-1434. Other references important in the present context are E. Gellhorn, The integrated action of the organism exemplified by studies on anoxemia, *Sigma Xi Quart.*, 25, 1937, 156-165; E. Gellhorn and A. Joslyn, The influence of oxygen want, hyperpnea, and carbon dioxide excess on psychic processes, *J. Psychol.*, 3, 1937, 161-168. Experimental procedures are related in *Amer. J. Physiol.*, 112, 1935, 519-528, 620-626, 662-668; 115, 1936, 679-684; 117, 1936, 75-78, and in E. Gellhorn and S. H. Kraines, Word associations as affected by deficient oxygen, excess of carbon dioxide and hyperpnea, *Arch. Neurol. & Psychiat.*, 38, 1937, 491-504. In his orientation, Gellhorn had the advantage of a general review of the subject in R. A. McFarland, The psychological effects of oxygen deprivation (anoxemia) on human behavior, *Arch. Psychol.*, 22, 1932, (no. 145), 1-135 (with 191 titles).

[3] A single instance of the great methodical difficulties encountered in the measurement of acuteness and range of apprehension of the sensible qualities of objects is A. D. Freiberg, 'Fluctuations of attention,' *etc.*, this JOURNAL, 49, 1937, 23-36, 173-197. With respect to the neurologist's approach to psychological problems, see T. A. Ryan, Neural resources of psychological performance, *ibid.*, 48, 1936, 193-220; M. Bentley, The psychologist's uses of neurology, *ibid.*, 49, 1937, 233-264 (esp. 235-245,

When experimenting upon those neural processes and states which suggest the 'phenomena' which they distantly call 'psychic' or 'mental' (usually prefixed by a 'so-called'), the physiologist and the functional neurologist are wont to imply that sensations and sensory differences are organic things to be plucked out of the body by the subject, who names them 'red,' 'green,' 'higher,' 'the same,' 'different,' and the like; but when something 'really psychical' is at hand (as in remembering, associating, and thinking), the physiologist then reluctantly brings in a ghostly mind, which proceeds to use its 'higher mental powers.'[4] An inevitable result of this misconception of matters psychological is that the determination of 'sensations,' 'discrimination,' and other 'sensory phenomena' is assumed to involve nothing more than the designation of a physical agent (stimulus), of a physiological effect upon the receptoral organ, a local stir-up in the brain, and a simple report upon quality, duration, difference, or the like. The psychological side of 'sensory' experimenting is, as a matter of hard fact, much more complicated than that, complicated by factors, over and above 'stimulus' and an *Elementarfunktion,* which are in play during the experimental occasion and complicated again by reason of the difficulties of expert and adequate report from the subject or observer.[5]

Beyond the problem of primary central effects of a reduction of oxygen-tension, however, Gellhorn has carried his researches into the realm of psychophysiological relationships, to the "fundamental problem as to how physiologic and psychic processes are interrelated."[6] There he seeks "to establish the dependence of psychic phenomena on physiologic factors by quantitative methods," "to determine the fundamental factors upon which the integrity of psychic processes depends, and to investigate whether any quantitative alteration in any one such factor alters both physiologic and psychic processes."[7]

Had Gellhorn been familiar with the history of experimental studies

260-261), The nature and uses of experiment in psychology, *ibid.,* 50, 1937, 452-469 (esp. 456-459), and Retrospect and prospect, *ibid.,* 51, 1938, 357-360. As a comment upon the physiologist's 'double-man' view of the organism, see M. Bentley, Mind, body, and soul in medical psychology, *ibid.,* 45, 1933, 577-591 (esp. 581, 586-588), and *The Problem of Mental Disorder,* 1934, 281-289.

[4] In combatting the opinion that some such 'psychic' agent as attention is responsible for the auditory effects of anoxia, Gellhorn argues for "changes in the nervous mechanism involved in the sensation itself" (*Amer. J. Physiol.,* 112, 1935, 524). It is to be doubted whether theories which set attention against sensation, with corresponding neurological speculations over 'seats' and 'elementary functions,' are any longer useful.

[5] Upon the difference between 'observer' and 'subject' in psychology, see comments by the present writer in this JOURNAL, 41, 1929, 682-683; 42, 1930, 320, and in his *New Field of Psychology,* 1934, 29, 270-274.

[6] *Amer. J. Psychiat.,* 93, 1937, 1413.

[7] *Ibid.,* 1414.

of psychophysical relationships, from E. H. Weber and G. Th. Fechner to the present, it is likely that he would have approached his experimental program with more sophistication and greater reserve.[8]

What really appears from the more 'psychological' studies of Gellhorn (when stripped of the indefinite and distant reference to 'psychic phenomena') is that the organism's capabilities in perceiving, distinguishing, and reporting-upon simple objects and their simpler aspects are infringed in certain ways and to a describable degree when respiration is modified *in a given experimental context;*[9] for in all such performances of the organism *context* must be taken into account quite as carefully as reduction of oxygen or increase of carbon dioxide. As every careful observer of the psychological operations of the body now knows, procedure and issue of organic activity rest quite as much upon the task set and upon the formal, occasional, and self-initiated instructions under which the body proceeds as upon the specific receptoral agent called 'stimulus' or upon the alveolar gases. Indeed stimulus taken without the whole occasion is a misleading abstraction.[10] An informing instance is the 'associational' experiment of Gellhorn and Kraines,[11] wherein the Wundtian method of associative verbal pairs, employing the Kent and Rosanoff tables, was used with variations of oxygen-tension, rate and depth of inspiration, and amount of inspired carbon dioxide.[12]

[8] A hint of the volume and range of the first half-century of this history may be had from the condensed summaries of problems, methods, and results in E. B. Titchener, *Experimental Psychology, Quantitative,* 2 vols., 1905. The last thirty years of the investigation of the interrelation of "physiologic and psychic processes" have been amply reviewed in recent psychological writings. Such physiological authors (*e.g.,* Sherrington, Barcroft, Bard, Cannon, and Ranson) as interpret their neurological results in mental or other psychological terms are always read by the informed psychologist in the light of this long history.

[9] When we go behind the many researches on anoxia which came out of aviation during and following the World War, we find that Gellhorn's type of study owes more than is commonly credited by physiologists and psychiatrists to the pattern of psychological experimentation set by Emil Kraepelin (1856-1926), who researched for more than forty years upon bodily conditions underlying the conduct of psychological tasks. For an account of Kraepelin's work see *The Problem of Mental Disorder,* 1934, 290-295. All functional neurologists and other physiologists who are ambitious to enter the field of the psychologist would do well to familiarize themselves with the nine meaty volumes of the *Psychologische Arbeiten* (1895-1927) as well as with current psychological methods, problems, and terminologies.

[10] O. F. Weber and M. Bentley, The relation of 'instruction' to the psychosomatic functions, *Psychol. Monog.,* 35, 1926, (no. 163), 1-15.

[11] E. Gellhorn and S. Kraines, *Science,* 83, 1936, 266-267. Cf. Gellhorn, *et al., J. Psychol.,* 3, 1937, 161-168; *Amer. J. Psychiat.,* 93, 1937, 1413-1434, and *Sigma Xi Quart.,* 25, 1937, 156-165.

[12] The Kent-Rosanoff list was adapted from S. Sommer, who supplied 66 of the 100 words. In choosing from this list, Kent and Rosanoff were careful "to avoid such words as are especially liable to call up personal experiences." See G. H. Kent and A. J. Rosanoff, A study of association in insanity, *Amer. J. Insan.,* 67, 1910, 37-96, 317-390 (esp. 37).

Gellhorn and Kraines seemed to find that a certain deficiency of oxygen (7.3%-11% oxygen in the inspired air) brought about "a marked shift in the type of response toward the more individual reactions,"[13]—the 'more individual reactions' being those which occur with relative infrequency in the Kent-Rosanoff lists, which were based upon 1000 'normal' individuals each responding to the self-same 100 stimulus-words. The infrequent words are presumably more directly related than the others to the life, experiences, and person of the particular responder. Gellhorn speaks of them as 'unusual' responses. He divides his results into the following four groups: O-Group, words which appear nowhere in the K-R 1000 responses to a given word (therefore wholly 'individual' words); 1-Group, words which appear there 1-15 times; 2-Group, words which appear 16-100 times; and 3-Group, words which are found in the tables more than 100 times (the most 'usual' words).

Since Gellhorn made the common mistake of assuming that the stimulus-word is the sole 'outside' determining element in the 'normal' associative occasion, he naturally infers that a 'shift toward the unusual' is to be accounted for by the variable tension of oxygen, the only variable in which his interest lies.

In order to demonstrate the presence and efficacy of other factors in the experimental occasion, we have repeated the method of paired associates as used by Gellhorn, only substituting for deficient oxygen some form of instruction—either formal, occasional or self-initiated—, an instruction, however, which may be, and frequently is, included without intent in such experiments. The psychological factor of instruction was designed in our experiments to direct the associative trend toward an 'individual' or biographical response.

In the earlier reports[14] of the Gellhorn experiments, the account is too meager for ready verification. From a somewhat fuller description[15] it appears that the distribution with respect to 'usualness of response' (see curves for control in chart on page 491) gives the ratio for the two halves of the experiment (*i.e.* first and second 50 stimulus-words). These latter form the points of reference from which departure is reckoned under change in oxygen or carbon dioxide. Since no individual measures are set down, it is again impossible to check the calculation. It looks as if two of the lines of the graph were rough measures of reliability and the other two (for O_2-lack and CO_2-excess) were indicators of a shift 'toward the left' in the distribution of responses.

As an assurance against discomfort and apprehension, this last article explains that an additional group of *Ss* (20 freshman students in medicine) was first given

[13] *Science*, 83, 1936, 267.
[14] *Science*, 1936, and *Amer. J. Psychiat.*, 1937 (as cited above).
[15] *Arch. Neurol. & Psychiat.*, 38, 1937, 491-504.

50 words without the Douglas bag and then 50 words with the bag but with the (false) statement that *S* was now subjected to an oxygen-deficit. In spite of the suspicion of a time-error and of the (probable) discovery by *S* that neither inconvenience nor danger awaited him (notwithstanding the experimenter's untruth), the authors are convinced that "these control experiments prove beyond doubt that

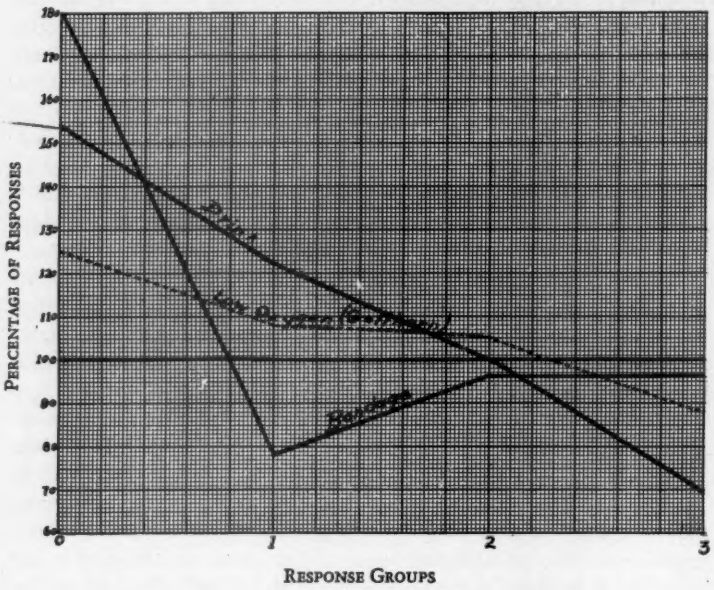

FIG. 1. EFFECT OF OCCASIONAL INSTRUCTION UPON RESPONSES
The percentages are expressed in terms of the Control experiments, the results of which are expressed by the solid line at 100. 0 = individual words, *i.e.* response-words not found in the Kent-Rosanoff tables; 1 = response-words found from 1-15 times; 2 = response-words found from 16-100 times; and 3 = response-words found over 100 times.

the shift to the left is the result of a characteristic effect on word associations of the gases inhaled."[36]

Using the same K-R word-list, we substituted, then, for Gellhorn's changes in the respiratory air a modification of the experimental occasion which promised to produce a similar effect upon verbal responses. In our first set of experiments, *O* was supplied with a small beaker containing slightly tinted water.[37] To the instruction to pronounce the first word which followed the list-word, was added the requirement

[36] *Ibid.*, 495.
[37] This experiment was carried out in the Psychological Laboratory of Cornell University under the able direction of Dr. Mary S. Ryan.

that, at a preliminary 'now,' a sip of the water be drunk. Fifty of the K-R words were orally given, each after a sip by *O*, and the other (equivalent) fifty words without the drink.

Our results (Fig. 1 and Table I) are cast in the same form as Gellhorn and Kraines,[18] save that our control results (no drink) are set in Fig. 1 as a base-line of 100% and the drinking results are related to it. This does not significantly change the curve. The four abscissal entries (0, 1, 2, 3,) refer to the groups characterized above.

TABLE I

DISTRIBUTION OF RESPONSE WORDS IN 'DRINK' AND 'CONTROL' SERIES

O	0		1		2		3	
	Drink	Cont.	Drink	Cont.	Drink	Cont.	Drink	Cont.
C	7	11	15	5	12	11	16	23
D	15	4	12	12	14	13	9	21
L	12	7	12	14	13	14	13	15
N	9	6	14	12	9	10	18	22
Totals	43	28	53	43	48	48	56	81

0=Response-words not found in Kent-Rosanoff tables; 1=Response-words found 1–15 times; 2=Response-words found 16–100 times; 3=Response-words found over 100 times.

Exactly the same major difference appears here as in the Gellhorn experiments, *i.e.* more infrequent words (in the sense of the K-R tables) and fewer words from Groups 2 and 3 (above 15 times in the tables). With us the 'shift toward the left' is somewhat wider than Gellhorn's.

From a scrutiny of our response-words, taken together with the methodical reports of our *O*s, it appears that the preliminary sip of colored water, taken just before the verbal pairing, inclined the organism toward a biographical reference. As a result, many of the response-words openly referred to *O*'s perceptive, memorial, and emotive situations in drinking something which had a medicinal look, while still other words appeared from the reports to be only less obviously connected with the same theme.

Ryan and Werner have explored the conditions under which biographical reference, centering in *somatic objects* and other varieties of *self-object* are created by the organism, as well as the numerous kinds and classes into which these objects fall.[19] Since these objects are autobiographical, it follows that the verbal representatives of them, in the method of paired associates, fall almost exclusively under the K-R designation 'individual responses,' *i.e.* outside the community of reference for such a human group as K and R's 1000 *S*s.

Everyone who has observed the ease with which current topics of interest (war, a flight to the stratosphere, the world's series in baseball, or a catchy tune) litter

[18] *Science, op. cit.*, 266.

[19] H. C. Werner, A descriptive study of somatic objects, this JOURNAL, 51, 1938, 225-234; M. S. Ryan, An experimental approach to the self-object, *ibid.*, 235-252.

the avenues of informal conversation will have no surprise for the outcome of the writer's injection of the stimulus-word 'turkey' into a series of K-R words given through several years to some thousands of students in a certain lecture-course. When this word was given just before the great November feast, the most-frequent response-word was 'Thanksgiving,' a term which rarely appeared when no feast was at hand.

In a second set of experiments the colored drink was replaced by a bandage so closely bound about *O*'s upper arm that circulation was slightly impeded in arm and hand. The general procedure was the same as before, save for necessary modifications in the formal and occasional instructions. The response-words were distributed as follows with respect to their 'unusualness' in the sense of the K-R tables.

TABLE II

DISTRIBUTION OF RESPONSE-WORDS IN 'BANDAGE' AND 'CONTROL' SERIES

O	0		1		2		3	
	Band.	Cont.	Band.	Cont.	Band.	Cont.	Band.	Cont.
J	2	4	7	9	11	14	30	23
G	6	2	7	12	16	10	21	26
M	13	5	10	15	12	13	15	17
W	15	10	12	10	13	17	10	13
Totals	36	21	36	46	52	54	76	79

A significant difference among our *O*s came out in the reports. *J* reported that the bandage had neither disturbed nor distracted him, while *G*, *M*, and *W* remarked that the constriction frequently brought the turgid arm and hand into the field of perceived objects and events. It concordantly appears from the figures in the 'bandaged' columns that the last three *O*s together returned twice as many O-group or 'individual' responses under a noticeable constriction as they returned in the corresponding control series (34 and 17), whereas *J* shows no consistent differences in distribution throughout the set. It is also worth noting that for *G*, *M*, and *W* a high percentage (nearly one-half of the excess of 17) was drawn from the adjacent 1-group, suggesting (albeit more results are called for) that it is especially the verbal occasions for the *relatively* rare response-words (to be found 1-15 times in the standard tables) which are here susceptible to the experimental occasion, the occasion which tends to weight the associative trends in the direction of the setting and of the bodily state and condition of the observing organism. Anyone who has babbled of the pleasures of the table when hungry or who has inclined toward pessimistic comment when tired need not be told that the injection of a disturbed bodily self into the perceptive, memorial, or emotive field

is as likely a determining condition of verbal formations as is a hypothetical change in excitability of the cerebral cortex by way of a diffused chemical agent. The relevance of our alleged factor is further supported by the frequency with which *O*'s expansion of his verbal reports revealed a reference to the bodily self, its state, and its setting, at the moment when the stimulus-word was presented.[20] At all events, it is plain that our control of the experimental situation and of the temper and tuning of *O* has brought about a further deflection of responses toward the unique and individual than occurred under the reduction of oxygen-tension in the researches referred to.

It appears likely that the Gellhorn experiments would profit by further checks and controls, especially since nothing short of a surgical or legal threat to bodily or social integrity is better designed to evoke an inquisitive, rebellious, or apprehensive reference to the various forms of the empirical 'self' than an interference with breathing. One has only to recall how the best and most courageous aces of the air trembled, reddened, and agonized before a simple test upon the rebreathing apparatus in the Medical Research Laboratory or how one was oneself disturbed, uncomfortable, and decidedly self-referring when O_2-tension dropped in the army-experiments to realize how ambiguous is the relation between respiration and 'the higher mental processes.'

The tendency toward the creation of somatic and other self-referring objects under low oxygen is abundantly exemplified also in the experimental field in which Gellhorn's own studies lie. Here are instances.

Wespi, who used the questionary, word series, verbal memory, perceived pictures, reaction-times, cancellation, paired associates, addition, and writing from dictation, found under low oxygen an excessive number of self-referring (*egozentrische*) and situational (*situationskonstellierte*) response-words, as well as 'senseless' syllables (rhyme, alliteration, repetition).[21] He found, as others had, great

[20] The popularity of the method of verbal associates derives, without a doubt, from its apparent simplicity and from the ease with which results are obtained, without special training of experimenter and subject—results which appear on their face to be of vast importance in psychiatrical, psychoanalytical, and criminological diagnosis. Like all other experiments involving verbal formulation, these really demand great skill in operation and high reserve from the interpreter. For cautions against the inexpert use and the illicit interpretation of the associational methods, see M. Bentley, *The Problem of Mental Disorder*, 1934, 298-300. The assumption that associational experiments with words evoke 'the higher mental processes' requires examination. As a matter of fact, the psychological facts commonly involved are of an extremely simple, automatized sort. They are much like the trigger responses of the finger on the release key in highly practiced simple reactions to a repeated sound or light. The unwarranted assumption often tempts the neurologist to bizarre theories of the cerebral and psychical processes involved.

[21] H. Wespi, Ueber psychische Insuffizienzerscheinungen bei vermindertem Luftdruck, *Arbeitsphysiol.*, 7, 1934, 484-516 (esp. 502-505).

numbers of external (*äussere*) associations, indicating that the experimental occasion —the perceptive-memorial-emotive 'present' of the observer—was here determinative, rather than those *innere* conditions which dispose the organism toward the 'usual' non-biographical responses of the K-R tables. Moreover, Wespi's subjects reported notable change of mood and emotive tuning (gaiety, intoxication, apathy) from which this experimenter inferred (as Gellhorn also has) an immediate derivation from low oxygen. The more careful students of organic effects of alcohol and drugs have taught us temperance in that direction!

Johnson and Paschal likewise speak of a "noticeable disturbance" of the subject, which declined as the experiments progressed.[23] These experimenters, who were alive to possible complications from the whole occasion in which the subject was imbedded, well knew, however, that the final emotive adjustment which the subject sometimes made did not eliminate the effective factors, leaving a fixed and static organism, on the one hand, and a simple change in cerebral excitability, on the other. The increase in functional output, for example, often observed with high oxygen-deficit in the re-breather, they laid to a self-pacing effort to overcome a handicap from the depleted air.[24]

Lowson[24] too found many body-objects and body-states incidental to exhilaration, irritable mood, lack of composure, fear, headache, muddlement, etc., under anoxia, all moments woven into the experimental occasion and all designed to affect both the ability of *S* to report and the character of the report itself. It may well be observed here that Gellhorn himself notes, under "somatic symptoms" (sic), such body-states and body-objects as were reported 'dizziness,' 'feeling of warmth,' 'decrease in hearing and seeing,' 'fulness in the head,' 'sleepiness,' and 'ringing in the ears.'

The same sort of issue of experimentation appears under the heading of 'complaints made by the subject' in a recent study of anoxia by McFarland.[25] Frequent complaints freely voiced by *S* include headache, respiratory changes or difficulties, excessive sleepiness, dizziness, difficulty in concentrating, sensory impairment, lassitude, indifference and fatigue. A list of 30 frequent complaints "observed in previous experiments" was also checked by each *S*.[26]

The main observation pertinent to all these reportable matters incident to experiments upon deficiency of oxygen, excess of carbon dioxide, compensation of anoxia by carbon dioxide, deep breathing, and tasks done with incentives to autobiographical reference, is that the experimental situation includes, for the performing and accomplishing *S*, a large group of psychological factors which notably affect performance and output, although they

[23] H. M. Johnson and F. C. Paschal, Psychological effects of deprivation of oxygen: Deterioration of performance as indicated by a new substitution-test, *J. Psychobiol.*, 2, 1920, 193-236.

[24] Gellhorn may have overlooked this common increase in effort and intent when he announced a "supernormal phase" of accomplishment at the end of the anoxic period. See his accounts in *Amer. J. Physiol.*, 112, 1935, 522; 115, 1936, 682.

[24] J. P. Lowson, The effects of deprivation of oxygen upon mental processes, *Brit. J. Psychol.*, 13, 1923, 417-434.

[25] R. A. McFarland, *The Effects of Oxygen Deprivation (High Altitude) on the Human Organism,* Department of Commerce, Bur. of Air Commerce, Safety and Planning Div., Washington, Report no. 13, 1938.

[26] *Ibid.*, 44-45.

may be only incidentally connected with the processes and sequels of respiration.[27] This observation does not tend to deny that there are immediate and direct effects upon bodily mechanisms which follow quantitative changes in respiratory air. It does go to show, however, that no changes in organic status or in functional output (whether or not the "so-called psyche" is invoked) can be adequately or safely described or explained unless these fundamental conditions and factors are competently taken into account.

A large segment of physiological research seeks to correlate environmental objects, processes, and agents with consequent modifications within the organism. Thus is the energy-change at the receptor related to receptoral and neural processes which are released by stimulus; and thus are the atmospheric gases related to subsequent respiratory, circulatory, neural and other processes within the body. In both these types of investigation, progress is limited by our exceedingly inadequate knowledge of the functions of the central nervous system, especially of the cerebrum. Neural physiologists are wont to supplement this scanty knowledge of cerebral functions (1) by invoking speculative doctrines of local and integrated mechanisms and operations of the brain and (2) by interpolating psychological 'phenomena' into their bodily accounts, the interpolation most commonly guided by a dualistic notion of 'mind' as running parallel to brain-process.

Both devices are dangerous: current theories of cerebral functioning are amply sustained by tradition but badly supported by fact, while psycho-

[27] In other experiments (addition of numbers, memory, handwriting, cancellation, descrimination, etc.) Gellhorn found similar 'psychic' effects of anoxia, which he believed to be due to a change in cerebral excitability, *i.e.* a primary result of alveolar deprivation. Had he not been so certain of his neural theory and so impressed by his alleged exposition of a complete "psycho-physiological" parallelism, he might have been more observant of the factors which we have noted. Something of this sort was strongly hinted at by his critics (Olkon, Neymann, Solomon, and Pollock) after the reading of one of the associational papers (Gellhorn and Kraines, *Arch. Neurol. & Psychiat.*, 38, 1937, 502-504). The fact that the same kind of 'psychic' damage was wrought by an excess of CO_2 and under hyperpnea also increases the difficulties which any simple 'excitability' theory encounters. Again, relief upon the addition of 3% CO_2 in the anoxic condition requires further examination of the situational factors which we have demonstrated. The "subjective symptoms" of headache, dizziness, fullness of the head, etc., which Gellhorn found in anoxia, but relieved on the addition of CO_2 (*Amer. J. Psychiat., loc. cit.*, 1429), should be regarded as something more than 'symptoms' and 'complaints.' They probably played an important part in the entire experimental setting and in the accomplishment of the subject.

In the text above, the phrase "psychological factors which notably affect performance and output" may disturb the neurologist, who is, as a rule, organicist, parallelist, interactionist, epiphenomenist, or believer in a dynamic psyche, as his mixed traditions and the immediate occasion may together suggest. 'Psychological' is not to say 'mental' in any existential sense, since the body is to be regarded as the only energetic agent and effective substrate to performance, whether performance is referred for description to physiologist or to psychologist.

logical interpolation is hopeless when couched in the gross terms of elementary sensation, higher mental process, and other 'psychic phenomena.'

In looking for the main cerebral and psychological consequents of deficient alveolar oxygen, no one could object to Gellhorn's desire "to tear down the borders" between physiology and psychology *provided* no relevant facts and factors are ignored and *provided* methods competent to a full description are employed.[28] It is deficiency in both of these provisions, however, that has been dwelt upon in the present discussion. Unless and until competence on both sides is united in the same investigator, it would seem to be expedient for the neurologist to restrict himself to a descriptive account of the relevant bodily processes treated in a physiological way, and for the psychologist to proceed in a psychological manner, drawing from general physiology and from functional neurology only the best authenticated facts and the soundest principles of bodily mechanism and operation.[29]

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[28] "To tear down the borders existing between psychology and physiology and to show by quantitative measurements that alterations in any physiologic conditions leading (etc.) . . . alter 'psychic' phenomena in a similar fashion," *Sigma Xi Quart., loc. cit.*, 157. Cf. *Amer. J. Psychiat.*, 93, 1937, 1433.

[29] M. Bentley, The psychologist's uses of neurology, this JOURNAL, 49, 1937, 233-264 (esp. 261).

AGE AND THE EFFECTS OF ROTATION

By SALVATORE RUSSO, Auburn Collegiate Center, and KARL M. DALLENBACH, Cornell University

It is commonly believed that children enjoy activities involving bodily rotation (riding on the merry-go-round, whirling on the piano stool, or spinning on the heels) and that adults dislike and avoid such activities. Holsopple, writing upon this point, states that "children whirl themselves violently about, bringing nearly all their canals into a position susceptible for stimulation, and are merely amused by the effects which would not be tolerable to an adult."[1] Apparently what is pleasurable for children becomes unpleasant for adults. At some time, therefore, during the transition between childhood and adulthood the affective character changes. When specifically does the affective change take place? Does it appear gradually or suddenly? Is there a period or age of indifference? Our purpose in undertaking the present study was to obtain answers to those questions. At the same time we also took the opportunity to note the uniformity at different age-levels of two phenomena incidental to rotation, past-pointing and the falling-reaction.

METHOD AND PROCEDURE

The *Ss*, 213 in number and ranging in age between 5 and 21 yr., were drawn at random from the public schools and the Collegiate Center of Auburn, N.Y.[2] Participation was voluntary, a free response to a request for subjects in a psychological experiment. The character of the experiments was not divulged until the *Ss* reported at the laboratory. This precaution was taken to insure an unbiased group, since persons having a dislike for rotation might, with prior knowledge, have been disinclined to volunteer. For the same reason we asked the *Ss* not to discuss the experiments with other pupils who had not as yet served. That we received their cooperation in this matter is shown by the fact that none of the *Ss* knew what to expect when he came to us.

The *Ss* reported at the laboratory between 3–4 P.M. on school days in groups of 3 or 4. Their height and weight were measured so as to afford an index of physiological age. Then they were taken one at a time to the experimental room, which was equipped with a Cornell rotation table.[3] To allay uneasiness, the experiment was first explained and then demonstrated by being performed upon the experimenter.[4] *S*,

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[1] J. Q. Holsopple, Space and the non-auditory labyrinth, in *The Foundations of Experimental Psychology*, 1929, 427.

[2] Our thanks are due to Dr. C. J. Hetherington, Superintendent of the Auburn schools, for his coöperation in affording us access to the *Ss*, and to Mr. Joseph Corcoran, who served as an assistant in this study.

[3] K. M. Dallenbach, An inexpensive rotation table, this JOURNAL, 42, 1930, 637.

[4] S. Russo, the junior author. Within a short time he became habituated to rotation and it had no effect upon him.

therefore, saw for himself that nothing harmful would happen to him, and he also knew by instruction and example what the procedure would be.

After receiving these instructions, *S* was blindfolded and placed in an upright position in the chair upon the rotation table, and the table was rotated. When the rotation was brought to a sudden stop, his behavior was noted and his report taken. If he did not offer a report upon affectivity, he was questioned specifically regarding it. After a few minutes' rest, the procedure was repeated with rotation in reversed direction. The first direction of rotation was alternated with every other *S*. Neither direction, consequently, had a positional advantage in the group of *Ss* as a whole.

This experiment was followed by two tests of past-pointing and two tests of the falling-reaction. In the tests of past-pointing, *S*, in accordance with instructions, opened his eyes when rotation was interrupted, struck at a target held at arm's length directly in front of him, and then held his arm loosely allowing it to 'drift.' The error in pointing and the drift were measured and recorded. Head movements were also observed and recorded.

In the tests of the falling-reaction, rotation was sagittal (head on shoulder or on knees facing sideways) and lateral (chin on chest with face down or head back with face up). When the rotation ceased, *S* brought his head and body to the normal posture. His movements were recorded together with his report.

Rotation was constant in duration and speed: 10 turns at the rate of 1 turn in 2 sec.[5] While executed manually, practice and the aid of an assistant who checked the turns with a stopwatch gave it a high degree of uniformity.

We first sought, with 49 *Ss*, to discover the critical age-level by bracketing. We started at ages 9 and 16 yr. and then gradually widened the bracket to 5 and 21 yr. When it became evident that there was no age-level within this range at which all the *Ss* agreed upon affectivity, we abandoned that objective and turned to the investigation of the adolescent years—ages 10–16 yr. inclusive. Within those age-limits we experimented upon 164 *Ss*.

Results

(1) Affectivity. The affective reports are classified in Table I, which shows—for sex, age-level, and direction of rotation—the number of *Ss* reporting pleasant (*P*), indifferent (*I*), or unpleasant (*U*).

(a) Age-difference. The most important results revealed in Table I are: (i) rotation is rarely reported as indifferent—in only 8% of the experiments; and (ii) rotation is not uniformly affective at any age-level. Both of these results confirm our initial experiment. The belief that all children enjoy rotation, and that all adults dislike it, is not borne out in this study. It is a false generalization perhaps based on the fact that more of the younger than the older children enjoy the experience: among our *Ss* 52% reported *P* at the 10-yr. level and only 25% at the 16-yr. level.

[5] The speed at which R. Bárány states he obtained the best results (Weitere Untersuchungen über den vom Vestibularapparat des Ohres reflektorisch ausgelösten rhythmischen Nystagmus und seine Begleiterscheinungen, *Monat. f. Ohrenhk.*, 41, 1908, 497.

The table shows furthermore that pleasantness tends to decrease with age and indifference to increase, while unpleasantness, after an initial sudden increase, tends to remain stationary. There is a strong indication that the affectivity of rotation shifts at the 11-yr. age-level. At the 10-yr. level, reports of pleasantness and unpleasantness practically balance each other. After that age, *P* predominates over *U* in a ratio of about 2 to 1. The only

TABLE I

AFFECTIVE REPORTS DISTRIBUTED ACCORDING TO CHRONOLOGICAL AGE

Age (in yr.)	Rotation	Boys			Girls			Total			Percentage		
		P	*I*	*U*	*P*	*I*	*U*	*P*	*I*	*U*	*P*	*I*	*U*
10	Clockwise	7	0	3	5	0	7	12	0	10	55	0	45
	Counter	5	0	5	6	0	6	11	0	11	50	0	50
	Total	12	0	8	11	0	13	23	0	21	52	0	48
11	Clockwise	4	1	7	4	0	5	8	1	12	38	5	57
	Counter	2	0	10	4	0	5	6	0	15	29	0	71
	Total	6	1	17	8	0	10	14	1	27	34	2	64
12	Clockwise	8	1	12	6	3	5	14	4	17	40	11	49
	Counter	8	1	12	5	2	7	13	3	19	37	9	54
	Total	16	2	24	11	5	12	27	7	36	39	10	51
13	Clockwise	4	2	6	2	1	6	6	3	12	29	14	57
	Counter	3	2	7	1	1	7	4	3	14	19	14	67
	Total	7	4	13	3	2	13	10	6	26	24	14	62
14	Clockwise	7	0	12	4	0	7	11	0	19	37	0	63
	Counter	7	0	12	1	3	7	8	3	19	27	10	63
	Total	14	0	24	5	3	14	19	3	38	32	5	63
15	Clockwise	3	1	8	4	0	9	7	1	17	28	4	68
	Counter	4	2	6	4	1	8	8	3	14	32	12	56
	Total	7	3	14	8	1	17	15	4	31	30	8	62
16	Clockwise	0	2	3	3	1	1	3	3	4	30	30	40
	Counter	0	1	4	2	1	2	2	2	6	20	20	60
	Total	0	3	7	5	2	3	5	5	10	25	25	50
Grand total	Clockwise	33	7	51	28	5	40	61	12	91	37	7	56
	Counter	29	6	56	23	8	42	52	14	98	32	8	60
	Total	62	13	107	51	13	82	113	26	189	34	8	58
%	Clockwise	36	8	56	38	7	55	37	7	56			
	Counter	32	7	61	32	10	58	32	8	60			
	Total	34	7	58	35	9	56	34	8	58			

exception occurs early at the 12-yr. level, where the ratio is decreased to about 5-4 (51-39).[6]

(b) Maturational difference. All individuals, it is well known, do not reach the same degree of physical development at the same age. Some mature early, others late. It is possible, therefore, that the affective shift is

[6] We have no knowledge of when or how the change occurs in the development of a given individual. That is another problem which well deserves investigation. A group of young children, selected because they like rotation, should be tested at intervals over a course of several years; and the affective shifts, if and as they occur, should be correlated with physiological as well as chronological age. We cannot safely infer from our group-results to the individual.

more closely correlated with physiological than with chronological age. To test that possibility we correlated the affective reports with height and weight—the only indices of physiological age available to us. These correlations, computed separately for each sex and for the group as a whole, were however little better than chance. Except for the extremes in weight, the affective reports are practically uniform in the different quartiles of height and weight (see Table II). Since the extremes in weight correlate with chronological age (the light *Ss* being younger and the heavy, older) the affective differences in these two weight-groups are merely chronological differences over again. Hence we feel justified in concluding that neither physiological nor chronological age alone accounts for the affective change.

TABLE II

AFFECTIVE REPORTS DISTRIBUTED ACCORDING TO THE *Ss'* HEIGHT AND WEIGHT

Report	Height (quartiles)				Weight (quartiles)			
	1st	2nd	3rd	4th	1st	2nd	3rd	4th
P	41%	33%	28%	36%	40%	35%	40%	22%
I	6%	10%	9%	7%	5%	6%	7%	13%
U	53%	57%	63%	57%	55%	59%	53%	65%

Its cause must be sought among the adventitious factors in individual development.

(c) Sex differences. The differences between the sexes are insignificant. The affective reports of the boys and girls are, as Table I reveals, very similar at every age-level, and the totals show practically identical percentages of *P, I,* and *U* reports. Of the 91 boys, 34% reported *P;* 7% *I,* and 58% *U;* of the 73 girls, 35% reported *P,* 9% *I,* and 56% *U.* Whatever it is that causes the affective shift, it must be something to which the two sexes are equally susceptible and sensitive.

(d) Differences in direction of rotation. The differences in affectivity between clockwise and counter-clockwise rotation were found (Table I) to be slight. Clockwise rotation was, however, reported as *P* more frequently and as *U* less frequently than counter-clockwise rotation. The differences, though small, are consistent for *P* and *U* in both sexes. Handedness, though not recorded, can in no way be responsible for these results, since the differences between the two directions of rotation are not of the order of the handedness-differences in the general population. We have no explanation of the rotational differences. Their significance for the entire group is slight: the *P* difference of +9 and the *U* difference of —7 being only 82 and 77 times chance respectively.[7] It may well be, therefore, that

[7] The two arrays treated by the method of χ^2 yields a P of 0.33. (Cf. R. A. Fisher, *Statistical Methods for Research Workers,* 1928, 82 ff.)

despite the consistency in the distribution of the results among the two sexes we are dealing here with chance.

(2) Corollary experiments. (a) Past-pointing. All of our 213 *Ss* participated in the post-rotational pointing-experiments and they all, it should be remembered, served without prior knowledge. Of these 183 (86%) yielded the usual results; they past-pointed in the direction of rotation. Of the 30 (14%) not conforming to this rule, 20 cross-pointed (*i.e.* pointed in the direction opposite to that of the rotation) when rotation was clockwise, 12 cross-pointed when it was counter-clockwise, and 2 cross-pointed after rotation in both directions.[8]

(b) Arm-drift. The direction of the drift of *S*'s arm after pointing subsequently to the stroke at the target agreed in every case with the direction of the error in pointing. If *S* past-pointed, he past-drifted; if he cross-pointed, he cross-drifted. The two types of post-rotational arm movement (pointing and drifting) are apparently both conditioned by the same or similar determinants.

(c) Head-movements. Compensatory head-movements, frequently observed in animals,[9] were noted in 68 (32%) of our *Ss*, who turned their heads in the direction opposite to that of the rotation. Of the remainder, 85 (40%) did not move their heads at all, and 38 (28%) moved it in the direction of rotation.

(d) Falling-reaction. Only 124 of our *Ss* participated in the falling-reaction experiments. Of these, 107 (87%) gave the usual results; a perception of falling in a direction negative to that of rotation, and a compensatory movement in the positive direction.[10] The remaining *Ss* (17 in number, or 13%) gave discrepant results:[11] either the *perception* of falling was lacking, vague, uncertain, or in an unusual direction; or the *compensatory movement* was lacking, faulty, or in an unusual direction. For 5 of these 17 instances, abnormal conditions in the ears were definitely responsible.[12] The others are probably to be explained in the same way, but

[8] Hastings obtained similar aberrant results. "Out of 700 applicants," he states, "47 cross-pointed with one or both arms on turning (Reactions of the normal labyrinth, *J. Otol., Rhinol., and Laryngol.*, 27, 1918, 486).

[9] B. C. Gruenberg (Compensatory motions and the semi-circular canals, *J. Exper. Zool.*, 4, 1907, 447-467) reports that the frog turns its head away from the direction of rotation, and S. S. Maxwell (*Labyrinth and Equilibrium*, 1923, 16) states that the frog and pigeon turn their head in the direction opposite to that of rotation.

[10] I. H. Jones, *Equilibrium and Vertigo*, 1918, 8.

[11] Hastings obtained similar results. He writes that "the falling reaction was found to be abnormal 11 to 25 times on turning right, and 10 in 254 times on turning left" (*op. cit.*, 486).

[12] To suppurating ears that had to be lanced, to mastoid operations, and, in one case, to infantile paralysis with spinal curvature and asymmetrical development of the right side.

no confirmatory evidence is at hand since the *Ss* in question did not report any auditory anomalies and an otologist was not available for aural examination.

Summary

The results of this examination of 213 *Ss* from 5 to 21 yr. of age to determine the effects of rotation may be summarized as follows.

(1) Rotation is rarely indifferent and it is not uniformly affective at any age-level.

(2) A greater proportion of young than of old children like the experience.

(3) The transition from liking to disliking in the rising age-groups is marked at the 11-yr. age-level.

(4) The affective shift is not correlated with maturational changes as measured by height and weight, for neither of these indices correlates with the shift.

(5) The causes of the affective shift must be sought among the adventitious factors in development.

(6) The sex-differences are insignificant. The cause of the affective shift, consequently, must be something to which the two sexes are equally susceptible and sensitive.

(7) Differences in affectivity between clockwise and counter-clockwise rotation are slight and, though consistent, are probably due to chance.

(8) The large majority (86%) of the *Ss* past-point. A small number give anomalous results. Mostly they cross-point when rotation is in one direction and past-point when it is in the other. Only 2 *Ss* (1%) cross-point consistently after rotation in both directions.

(9) The arm, held loosely after pointing, drifts in the direction of the error in pointing.

(10) Compensatory head movements during rotation were made by only about a third (32%) of our *Ss*. The largest proportion (40%) did not move their heads and 28% moved it in the direction of rotation.

(11) The normal falling-reaction was reported by a large majority (87%) of our *Ss*. Some of the irregularities reported, and probably all, are due to anomalous conditions of the *Ss'* ears.

A PRELIMINARY STUDY OF THE RÔLE OF POSTURAL ADJUSTMENT IN CONDITIONING

By G. L. FREEMAN, Northwestern University

In an earlier discussion of the rôle of postural adjustments in the government of overt reaction,[1] the writer suggested that tonic response segments may come to anticipate or 'set' the associated phasic response in the process of repeated stimulation and 'conditioning.' This paper presents a preliminary attempt at experimental check of the hypothesis. Done on only one animal, the study is reported in the hope that laboratories primarily interested in the conditioning technique will be encouraged to continue what seems to be a promising line of attack.

Procedure. The experiments took the form of recording neuromuscular activity in the four limbs of a dog during various conditioned-unconditioned stimulus-combinations. The same unconditioned stimulus (shock) was used in all cases, but was applied to different paws accordingly as the conditioned stimulus was varied. Conditioned stimuli were four tones ranging from 100 to 1000 ~ and hereafter referred to as CS_1, CS_2, CS_3, and CS_4. Combinations of tone (3 sec.) and shock (0.2 sec.) were as follows:

CS_1-UCS, always applied to right rear paw;
CS_2-UCS, applied to right or left rear paw in a random order;
CS_3-UCS, applied to right or left rear paw or right front paw in random order;
CS_4-UCS, applied to right or left rear or front paw in random order.

By design the conditioned stimuli differed in the specificity of the response which they anticipated and our problem was to find out if the postural antecedents of the unconditioned limb movements would reflect the experimental variations. The animal was suspended in the conditioning harness and small galvanized iron shocking plates were attached to each paw. These plates also formed part of a delicate lever arrangement which marked on smoked paper the flexion-extension movements of each limb. In a few instances, 6 mm. gold-plated electrodes were cemented to the flexor muscles and connected through a suitable amplifier to a cathode ray oscillograph. A commutating switch permitted viewing of action potential phenomena from any one of the four limbs, but simultaneous recording was not possible. In this work, as in administering the electric shock to the animal, the 'indifferent electrode' was attached to the base of the tail.

In training, the individual trials were spaced about 30 sec. apart and at first the shock was invariably applied to the right rear paw (the CS_1-UCS combination). When conditioned reactions (tentative movements in the right rear limb) appeared, this procedure was interspersed with shocks to one or the other of the rear paws (the CS_2-UCS combination) and after differentiation in postural preparation appeared upon presentation of the appropriate conditioned stimulus, the training was further complicated by the addition of the CS_3-UCS combination in which a given

* Accepted for publication June 6, 1938. Studies in Muscular Tension, Series 2, no. 8.

[1] G. L. Freeman, The problem of set, this JOURNAL, 52, 1939, 16-30.

tone was indiscriminately followed by shock to any one of the four paws. Finally another tone (CS_3) was also introduced with shock to one of three paws. In all, the experiments were of over a month's duration, with the new discrimination problems added at the rate of about one per week.

Results. Since records are incomplete for the total training period, only approximate values can be given for the relative ease of 'conditioning' to our four experimental variables. Decreased anticipatory postural activity in *all* limbs except the one which received the shock appeared within the first 20 presentations of the CS_1-UCS combination. To induce postural differentiation between the specific CS_1 stimulus and the CS_2 stimulus which signalized shock to one of the rear paws took over a week of intensive training; further differentiation, as between the CS_1 or CS_2 stimulus and the CS_4 stimulus (shock to any one of the four paws) took even longer. The final addition of CS_3 (shock to one of three paws) never exhibited a specifically differentiated postural adjustment and the animal tended to respond to the tone either by tentative movements of the rear limbs (as to CS_2) or more generally, by movements of all limbs (as to CS_4). To a certain extent, the difficulty experienced with the CS_3 conditioned stimulus was probably a function of its placement in the training series; that is, had it been introduced following CS_1 the animal might readily have shown differentiation between preparation for shock to one of three paws and the other conditions.

Action potential records indicated some slight differentiation in response to CS_4 and CS_3 stimuli, but both here and elsewhere, the fact that the records were obtained from one muscle group at a time does not give them much weight. About all that can be said of this part of the experiment is in the suggestions which it carries for further work. Differing degrees of electrical activity in the four muscle groups occurred under the conditions studied, and the indication is that if these would provide a very sensitive index of antecedent postural adjustments if recorded simultaneously and with suitable controls. This would require either a set of four matched amplifiers or a system permitting a more rapid commutation of the signals through the final output stage than was used in this experiment.

The patterns of postural adjustment which accompany the presentation of different conditioned stimuli are most readily studied in connection with the kymograph records of limb movements, a few of which are shown in Fig. 1. Record A was taken on the animal during the third presentation of CS_1-UCS combination and indicates that as yet the animal has not come to respond to the tone as the signal for shock. Instead the activity in all limbs is continued prior to, during, and after the combination is applied. Especially important, however, as a forerunner of a conditioned postural reaction to the same stimulus is the heightened activity in the limb (right rear) which received the shock. This prolonging of the unconditioned reaction was characteristic in all situations, especially in the early stages of training.[2]

[2] Q. F. Curtis (Experimental neurosis in the pig, *Psychol. Bull.*, 1937, 34, 723) has found, however, that the unconditioned shock stimulus is followed by a period of comparative relaxation in the reacting member before anticipatory behavior to the next shock begins. It may be that our results are a function of the more complicated situation in which the animal was placed. We did observe, toward the end of training in the CS_1-UCS combination, a shift in the pattern of post-stimulation activity: that is, instead of a gradual reduction to the time of presenting the next CS-UCS combinations, the UCS seemed to serve as a trigger to discharge a mounting tension which began to build up again in anticipation of the next stimulus.

Record B shows a conditioned postural reaction to stimulus S_1 taken during the second week of the tests. It should be noted that the animal is now resting fairly quietly in the harness and that the conditioned stimulus S_1 is the signal for localized anticipatory tension in the part which is to receive the shock. The heightened flexor activity of the right front limb which follows the unconditioned response of the

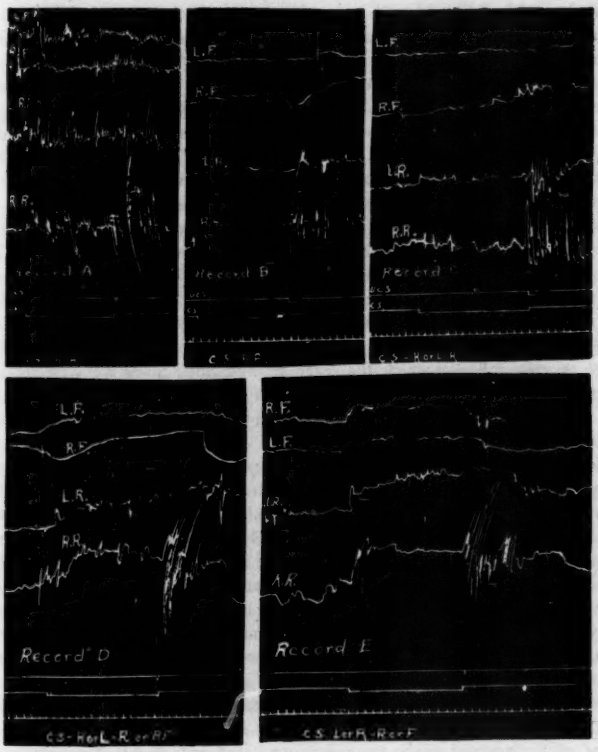

FIG. 1. PATTERNS OF POSTURAL ADJUSTMENT WHICH ACCOMPANY THE PRESENTATION OF DIFFERENT CONDITIONED STIMULI

right rear limb is perhaps a homolateral irradiation phenomenon. Occasional omission of the shock stimulus was found to have little effect on the fundamental character of this postural adjustment; that is, the anticipatory contraction of the right rear limb was followed at the appropriate time by more vigorous withdrawal movements.

Record C, taken after considerable training with the combination of CS_2 and shock to one of the rear limbs, shows greatest anticipatory activity in the hind quarters and indicates that the more non-specific the conditioned stimulus, the more

diffuse is the postural preparation. Also of considerable interest is the appearance of contralateral reactions as well as homolateral spread following administration of the shock stimulus.

Record D was taken near the close of the experiment and is the response of the animal to the CS_2 stimulus with shock delivered to one of three places. It will be seen that there is little differentiation between the anticipatory reaction of the left front limb, which definitely will not receive a shock, and the other limbs which may. Instead, the pattern of postural adjustment closely resembles that made to CS_4 (see next record).

Record E was taken the same day as Record D. The conditions differed only in that the signal (CS_4) for shock to one of four possible limbs was given. In response to it the animal shows anticipatory movements in all limbs with marked contractions in the one which actually receives the shock.

It should be mentioned that we have selected for comparative purposes only records in which the unconditioned stimulus was delivered to the right rear paw. Similar records could be exhibited in which the spread of anticipatory postural adjustments varied with the conditioned stimulus utilized and where the unconditioned shock was delivered to some other limb. During the final week of training the various stimulus combinations were alternated in fairly close succession, with the result that the animal became confused, lost postural differentiation and tended to respond in a generalized fashion to all tones. In fact, kicking became so violent in one instance as to completely wreck the lever arrangements. Since we were interested primarily in the demonstration of postural differentiation, we have left the study of its confusion under conditions demanding rapid intraorganic readjustments for detailed analysis at some future time.

Discussion. The existence of different postural antecedents in our several conditioning experiments suggests a 'set' explanation in the selection of the overt reactions which were made.[8] When the dog is first placed in the harness there is aroused an attitude of apprehension which expresses itself in terms of a generalized postural adjustment (see Record A in Fig. 1). The dog does not 'know' from what quarter to expect punishment or reward and is consequently prepared for a wide range of possibilities. This general postural adjustment is active when the combination of tone (CS_4) and shock (UCS) is first applied and continues in heightened degree in the bodily part which actually receives the shock (see Record A). Consequently when the conditioned stimulus is next applied it meets a deferential postural adjustment with major neuromuscular activity centered in the limb which received the unconditioned shock stimulus. Repetition of the stimulus combination results in the 'capture' of the postural set by the conditioned stimulus; that is, the tone becomes independently capable of arousing the postural adjustment as when given in a series with other stimuli which are not a signal for the application of the unconditioned stimulus to this particular limb (see Record B). This postural set is a conditioned response, preparing the organism for the unconditioned withdrawal reaction to shock. It differs from the unconditioned response in degree, and probably in char-

[8] For a summary of literature pertaining to the rôle of preparatory sets and tension in conditioning experiments see O. H. Mowrer, Preparatory set (expectancy): A determinent in motivation and learning, *Psychol. Rev.*, 45, 1938, 62-91.

acter as well,[4] and it may upon temporary removal of the unconditioned stimulus produce an appropriately timed conditioned withdrawal reaction.[5]

The experiments indicate that not all conditioned postural adjustments are equally limiting in the unconditioned responses which they prepare for and anticipate. Thus, when a conditioned stimulus (CS_2) is given which signals the forthcoming application of shock to one of the two rear limbs (instead of the specific CS_1 signal) the animal becomes 'set' with heightened tensions in those parts which may receive the shock (see Record C). Likewise, there is an attempt at differential postural adjustment when a conditioned stimulus (CS_3) signals a shock to one of three possible loci (see Record D). An even more generalized set anticipates a fourfold possibility (see Record E) and it differs from the generalized apprehensive posture only in its occurrence in response to a specific conditioned tonal stimulus (CS_4).

In our theoretical analysis of conditioning in relation to set phenomena, we hypothecated that the conditioned postural adjustment carried a different order of response component than did the overt withdrawal reaction. For this we argued as follows:

"The stimulus configuration (s_1 tone, S_1 shock) evokes a response pattern (r_1R_1) which has both tonic and phasic elements. In the resulting complex it is reasonable to assume that tonic and phasic segments may be captured by different parts of the stimulus configuration. The phasic element (R_1) will be associated with the most dominant part of the stimulus configuration (S_1) and its character will be determined by 'innate' connections. This is the unconditioned response of Pavlovian terminology. . . . There is a tendency for the postural adjustment of apprehension (r_1) to be associated with the remaining tone stimulus (s_1). But this s_1r_1 connection does not remain unmodified by the S_1R_1 connection, for under repetitive stimulation the posture of generalized apprehension is early changed into specialized adjustment in one part of the body. The pattern of anticipatory tension is now localized in the leg to be stimulated and the dog turns his head in that direction. This qualitative and quantitative shift in r_1 may be accomplished without special reference to s_1. What probably happens is that the tonic response element r_1 persists longer in that part of the body making the phasic reaction R_1, and that this differential posture carries over to the next application of the stimulus combination where its connection with the conditioned stimulus s_1 continues. . . . Since the tonic factor outlasts the phasic factor, it will at first be the only reaction component connected with the stimulus complex when the unconditioned stimulus S_1 is withdrawn. But since r_1 has been built up by training to be anticipatory of R_1 it may now activate this phasic response through proprioceptive channels. In this scheme the first effect of the tonic response r_1 is to

[4] Other writers have commented on the lack of similarity between conditioned and unconditioned responses; cf. E. G. Wever, The upper limit of hearing in the cat, *J. Comp. Psychol.*, 10, 1930, 231-232; C. C. Tolman, *Comparative Psychology*, 1934, 317-408; E. R. Hilgard and A. A. Campbell, Vincent curves of conditioning, *J. Exper. Psychol.*, 21, 1937, 310-319; E. R. Guthrie, *The Psychology of Learning*, 1935, 258.

[5] This type of anticipatory phenomenon has been noted in other work; cf. G. L. Freeman, The galvanic phenomenon and conditioned responses, *J. Gen. Psychol.*, 3, 1930, 524-539, and E. R. Hilgard, Conditioned eyelid reactions to a light stimulus based on the reflex wink to sound, *Psychol., Monog.*, 41, 1931 (no. 184), 1-50.

lower the threshold for the phasic response R_1 *via* proprioceptive afferents to the motoneurone field of R_1. Later, if r_1 is sufficiently intense it can itself supply a full substitute stimulus to give a weak approximation of R_1."[6]

While still adhering to the above account in principle, we see that the conditioned postural adjustment might also be regarded as a tentative approximation of an about-to-be overt withdrawal reaction.[7] In other words, this experiment is not a critical test either of the hypothesis outlined above, or of an alternate explanation which makes no distinction between tonic and phasic response elements.[8] Action potential records might with suitable controls indicate whether the conditioned 'set' of a muscle differs qualitatively as well as quantitatively from the reaction of that muscle to the unconditioned stimulus. No acceptable evidence for this assumption was found in the records which we obtained, but more adequate instrumental procedures might reveal such differentia. As they stand the results show that the more specific the stimulus-response connections in conditioning experiments, the more diffuse is the postural adjustment.

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[6] G. L. Freeman, *op. cit.*

[7] Mowrer (*op. cit.*, 73), however, thinks that while the postural set is modified by training, it is not fundamentally a learned phenomena but is due to "a tendency on the part of every reaction, once activated, to show following a relatively brief period of refraction, a more prolonged period of increased excitability."

[8] R. S. Woodworth, *Psychology*, 1934, 327; cf. also this JOURNAL, 51, 1938, 130.

AUDITORY MEMORY SPAN AS TESTED BY SPEECH SOUNDS

By VIRGIL A. ANDERSON, Stanford University

Memory span, defined as the number of discrete elements grasped in a given moment of attention and organized into a unity for purposes of immediate reproduction or immediate use, is determined in various ways.[1] One method is to determine the largest number of relatively discrete units that an individual is able to retain and reproduce immediately after one presentation. Various materials have been used, but the types most frequently employed have been digits, letters, short words, and nonsense syllables, with digits and letters predominating.[2]

While many of the investigators have dealt with the problem of memory span in general without reference to the particular type of mental imagery employed by the subjects, a number of them have attempted to construct a test that would involve one type of mental imagery in relative isolation.[3] The sensory faculty usually selected for study is either the visual or the auditory. It has apparently been assumed, however, that the method of presentation itself was capable of determining the kind of ideation involved in the test. Hence, visual presentation has been used to determine the visual span and auditory presentation for the auditory span.

Careful study of this problem, however, leads to the conclusion that other factors, in addition to the method of presentation, are operative in determining the type of ideation that the subject will use.[4] Among these factors are the nature of the material employed in the test and the method of reproduction. Previous experimenters have often failed to control these factors, having simply used digits, for example, with graphic reproduction for both the visual and the auditory tests. With material so readily visualized as digits, especially when reproduction is written, it appears safe to conclude that no valid claims can be made for auditory memory, even though the presentation may be oral.

Object. The purpose of the present investigation was to study the characteristics of the auditory memory span, and to determine what use is made of memory span and what part it plays in various mental activities, when stimulus-material of a predominantly auditory nature is used.

Procedure. Isolated speech sounds were selected as being the most difficult material to visualize and the most free from possible meaningful associations. When these sounds are given auditorially and are sounded, not merely named, and with oral reproduction, there is a minimum opportunity for visual imagery to function. With respect to some of the sounds of English, for example, it can be said that there

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[1] Twenty-seven different ways of determining the memory span are described by J. P. Guilford and K. M. Dallenbach in an article in which they propose still another way (The determination of memory span by the method of constant stimuli, this JOURNAL, 36, 1925, 621-628).

[2] For the various materials used in experiments on memory span see A. B. Blankenship, Memory span: A review of the literature, *Psychol. Bull.*, 35, 1938, 4 f.

[3] Cf. Blankenship, *op. cit.*, 5 f.

[4] V. A. C. Henmon, The relation between mode of presentation and retention, *Psychol. Rev.*, 19, 1912, 79-96.

does not exist, for the individual untrained in phonetics, a specific visual symbol which always represents that sound and that sound alone.

Accordingly, two tests were devised, the one employing eight English vowels and diphthongs; namely, [i] key, [æ] hat, [eɪ] say, [aɪ] lie, [a] father, [ɔ] awe, [oʊ] oh, [u] who, and the other test consisting of the six voiceless consonants, p, t, k, sh (she), s, and f. The tests were given separately. The sounds were presented strictly as *sounds*, with no reference to the alphabetical or other visual symbols which are used to represent them. They were sounded in a monotone without rhythm or accent at the rate of one per second with a short period of silence separating them so that each one formed a complete sound-unit in itself. Reproduction was immediate and oral. A total of 132 students from the University of Wisconsin were tested.

Methods of scoring. Two methods of scoring the reports of the *Ss* were used. One was the weighted-score method described by Peatman and Locke.[5] Briefly, this method consists in establishing a basal score, which is that series immediately preceding the first failure. To this basal score is added a unit or fraction of a unit for each success above it regardless of intervening failures. Since each of the present tests was composed of three group of series, one-third of a unit was added to the basal score for each success above it. In addition to the weighted score, a raw score was also computed by a method similar to that used for scoring the digit span tests found in the Stanford Revision of the Binet-Simon Scale.[6] That is, three trials were given for each length of series, and the raw score was taken as the longest series correctly reproduced immediately preceding the first failure on all three trials.

Results. (1) The span for consonants was found to be materially lower than that for vowels. The vowel test yielded a mean weighted score for the group of 4.73 and a raw score of 5.46. The mean weighted consonant score was 3.83 and the raw score was 4.66. All of these values are substantially below the commonly accepted norms for digits. No significant sex differences were found, the slight differences which did exist always being in favor of the women.

(2) A reliability coefficient of 0.86 was obtained for the vowel test from a retest of a college class of 30 *Ss* after a semester of phonetic training. The method was that of self-correlation using the Pearson Product-Moment formula. The reliability of the consonant test was considerably lower, only 0.31. While performance on the consonant test was definitely improved by the phonetic training, there being a difference of 0.20 between the mean weighted scores at the beginning and at the end of the semester, the vowel test scores remained relatively constant, showing a small loss, rather than a gain, of −0.05 between means.

These results indicate that the auditory memory span for vowel sounds is relatively independent of phonetic training. In the case of the consonant test, however, such training tends to improve performance but not proportionally for all members of a group.

(3) A correlation coefficient of $+0.47 \pm 0.046$ was found when vowel scores were compared with consonant scores. This was interpreted to reflect the possible influence of various extraneous factors which were discovered to affect performance

[5] J. G. Peatman and N. T. Locke, Studies in the methodology of the digit span test, *Arch. Psychol.*, 25, 1934, (no. 167), 1-35.

[6] L. M. Terman, *The Measurement of Intelligence*, 1916, 1-362.

on the consonant test to a greater degree than on the vowel test. These factors, which are only mentioned here by way of summary, include the influence of phonetic training on consonant scores mentioned in *(2)*, the relative ability of the *Ss* to make fine discriminations among speech sounds when hearing them pronounced, the greater readiness with which consonants are visualized by the average *S*, and the difficulty which some *Ss* experience in attempting to reproduce isolated consonant sounds orally.

(4) Pitch discrimination was found not to be related to auditory memory span for the group studied. This was indicated by the low correlation of +0.12 between scores on the Seashore Sense of Pitch test and weighted scores on the vowel sounds test for a group of 55 *Ss*.

(5) Within the limits of the group studied, there was no evident relationship between auditory memory span, as measured by these tests, and auditory acuity as

TABLE I

SHOWING RELATIONSHIP OF MEAN AUDITORY MEMORY SPAN SCORES TO YEARS OF FOREIGN LANGUAGE STUDY

Years Studied	No. Ss	Mean vowel scores		Years Studied	No. Ss	Mean vowel scores	
		Weighted	Raw			Weighted	Raw
1	1	3.00	5.00	6	21	4.70	5.45
2	10	4.90	5.86	7	23	5.06	5.76
3	17	4.20	5.04	8	12	4.86	5.58
4	13	4.56	5.33	9	4	5.16	5.75
5	25	4.49	5.32	10+	6	5.33	6.12

measured on an audiometer. Elaborate hearing tests were given to a group of 28 *Ss* included in the present study.[1] No defects of hearing were found among those *Ss* making the lowest scores on the memory span tests within this group nor did those for whom a slight hearing loss was discovered present the shortest memory spans.

(6) Significant evidence was discovered of a positive relationship between auditory memory span and ability in and learning of foreign language. In the first place, when the high school and college grades earned by the *Ss* in foreign language study were correlated with their weighted auditory memory span scores for vowels, the coefficient of correlation obtained from the Pearson formula was found to be $+0.34 \pm 0.05$. Since this r is almost seven times its probable error, some degree of correlation is indicated. It is not particularly high, however, and may reflect in part the fact that a still higher correlation was found between memory span scores and general university grade point averages, as will be shown in *(7)*. It should also be noted that foreign language grades are based upon written work as well as oral performance, and that school grades in general constitute at best a doubtful indicator of ability.

Of more interest and possibly more importance were the results obtained from a study of other items relative to the *Ss'* foreign language background and achievements. Table I shows a comparison between years of foreign language study and

[1] Merle Ansberry, Auditory deficiencies of acuity and their correction with prosthetic appliances, Ph.D. Thesis, University of Wisconsin, 1937.

the mean weighted auditory memory span scores for vowels made by those *Ss* whose total time of language study is represented by those particular years. It can be seen that with the exception of the groups represented by the second and seventh years, the relationship is very close. In any case, a high degree of correspondence is indicated.

One should not conclude from this, however, that foreign language study increases memory span, or even that it improves performance on the tests. Rather, the more likely conclusion appears to be that those individuals possessing superior auditory memory spans found foreign language study comparatively easy and interesting, received relatively good grades in it, and as a result took more years of it than those less fortunate students whose shorter memory spans, among other things

TABLE II

SHOWING RELATION OF MEAN AUDITORY MEMORY SPAN SCORES TO FOREIGN LANGUAGE BACKGROUND AND ACHIEVEMENT

Background and achievement		No. Ss	Mean vowel scores	
			Weighted	Raw
A. Foreign language spoken in in the home:	Yes	41	4.61	5.34
	No	91	4.74	5.51
B. Foreign languages learned:	Easily	47	5.00	5.73
	Average	57	4.59	5.36
	With difficulty	28	4.36	5.11
C. Study of foreign language:	Enjoyed	46	4.92	5.65
	Average	64	4.61	5.37
	Disliked	20	4.53	5.35
D. Speaks a foreign language:	Not at all	27	4.50	5.29
	With difficulty	56	4.70	5.52
	Fairly well	46	4.87	5.46
	Fluently	4	5.25	6.50

perhaps, made foreign language study more difficult and arduous. A number of facts appear to support this conclusion. In the first place, the results obtained from the retest of the group discussed in section *(2)*, after a semester of phonetic training, indicate that the study of a foreign language would likely have little effect upon the auditory memory span scores for vowels. Of similar import are the results obtained from a comparison of memory span scores made by *Ss* coming from homes in which a foreign language was spoken (in addition to English) with those coming from homes in which only English was used. As Table II shows, the mean scores made by *Ss* from bi-lingual families are actually lower than those made by *Ss* from mono-lingual families.

That memory span may on the contrary contribute to language ability, is further suggested by the remaining sections of Table II. In every instance, save one of the raw scores in Section D, there is a perfect correspondence between mean auditory memory span scores and indicated foreign language aptitude and achievement.

(7) Auditory memory span is positively related, but only moderately, to the capacity to do school work, to the extent that that capacity is indicated by school grades. A correlation coefficient, using the Pearson formula, of $+0.37 \pm 0.052$ was

obtained between weighted memory span scores for vowels and University of Wisconsin grade point averages.

(8) Auditory memory is also involved to a moderate degree in the complex of abilities measured by the University English Usage Examination, which the University of Wisconsin gives routinely to entering freshmen. When percentile rankings on the English Examination were correlated with the weighted scores on the vowel test, a coefficient of $+0.35 \pm 0.07$ was obtained.

(9) Auditory memory span for vowel sounds bears a slightly lower degree of relationship to general intelligence. Two measures of intelligence were used: The American Council Psychological Examination, which is routinely given to entering freshmen, and intelligence quotients. Percentile rankings on the first measure yielded a correlation coefficient with weighted memory span scores for vowels of $+0.307 \pm 0.058$. The second measure showed a correlation of $+0.325 \pm 0.059$.

Summary. On the basis of the findings brought out in this study, the following conclusions are indicated.

(1) The memory span for speech sounds is materially lower than the accepted norms for digits.

(2) The auditory memory span for speech sounds, particularly vowels, appears to be independent of phonetic training and a foreign language environment. The vowel sounds test was found to be highly reliable.

(3) Neither pitch discrimination nor auditory acuity is related to auditory memory, as measured by the vowel sounds test.

(4) A relationship is indicated between auditory memory span for speech sounds and ability in and learning of foreign language. The findings indicate that this relationship is in the nature of a partial dependence of language skill upon memory span.

(5) Auditory memory span is positively related, but only to a moderate degree, to school grades, knowledge of English usage, and general intelligence. It is worthy of note that the obtained coefficients of correlation with all of these measures are very similar.

(6) Speech sounds, and especially vowel sounds are highly desirable materials for a test of the auditory memory span.

EXTINCTION, SPONTANEOUS RECOVERY, AND FORGETTING

By G. H. S. Razran, Columbia University

If an extinguished conditioned response (CR) has recovered some of its lost ground after a lapse of a certain time, we usually assume that the amount of recovery is in some way directly proportional to the amount of extinction. If the CR has been only partly extinguished, another legitimate postulate appears to be that the quantitative total of the recovery is affected in a degree by some forgetting of the non-extinguished portion of the CR. A third contention that the recovery itself suffers some forgetting is also not without merit. To illustrate the possible workings of these three hypotheses: Let us suppose that a CR which had been extinguished to 40% of its normal value was retested after a lapse of 48 hr. and that during this time recovery was 50% of the amount of extinction and forgetting 25% of the amount of conditioning. Then, if we hold only to the first hypothesis, the recovered CR should be 70% of its normal value (40 plus 50% of 60). Add the second supposition and the result will be only 60% (minus 25% of 40), while the third assumption would further reduce the recovery by a small percentage (the precise amount depending upon the time curves of recovery and forgetting). Now, all this is not just a matter of theory, but is subject to rigid experimental verification. Either by keeping recovery time constant and varying the degrees of extinction or by varying time and holding extinction constant, we obtain data from which we may determine not only the number of constituent factors of spontaneous recovery but also their exact individual contributions. In the present experiment the first and easier method of inquiry was chosen; namely, recovery times was always 48 hr. and extinction ranged from 27% to 67%.

Method and procedure. All the 12 adult human subjects of this experiment previously had salivary CRs to various visual and auditory stimuli. The old CRs were, however, also previously extinguished and all the *Ss* re-formed new CRs to a red colored 15-w. light. The reconditioning was very fast and the new CRs were soon quite stable and regular, ranging from means of 198 to 316 mg. of net conditioned salivation in 1-min. periods. The *Ss* were then divided into 6 equated groups and each group was given a different number of extinction trials: 4, 8, 12, 16, 20, and 24. The intervals between extinction trials were 2 min.—1 min. of which was a control trial—and the extinction trials themselves were of 1-min. duration. All the CRs were retested, 48 hr. after extinction, for spontaneous recovery.

The method of measuring saliva and the writer's general CR methodology have been described before and will not be detailed here.[1] The saliva is measured by increments in weights of dental cotton rolls (Johnson and Johnson, No. 3, 0.5 x 1.5 inches) inserted under *S*'s tongue for short periods of time, usually 1 min. To prevent evaporation, scale-corrosion and absorption, the rolls are weighed in small envelopes and reweighed in the envelopes immediately after their removal from *S*'s

* Accepted for publication December 7, 1938.

[1] G. H. S. Razran, Conditioned responses: An experimental study and a theoretical analysis, *Arch. Psychol.*, 28, 1935 (no. 191), 1-124; A simple technique for controlling subjective attitudes in the salivary conditioning of adult human subjects (in press, *Science*).

mouth. The general methodology of the CR involves ruling out interfering subjective attitudes by misinforming the *Ss* about the true purpose of the experiment. As a rule this can be achieved only when a *series* of stimuli is presented during a comparatively long eating period (2-4 min.) and not with the traditional single-stimuli-plus-brief-reënforcements method. In the present study, the eating periods were 2 min. in length during the course of which the 15-w. lights were presented intermittingly 48 times (24 times in the 1-min. test-periods). The *Ss* were misinformed about the purpose of the study by being told that it was "to find out the effect of eye-fatigue upon digestion." The experiment was conducted in a fairly soundproof room, between 3 and 5 P.M. In no case did any *S* show any evidence that he "caught on" to the true purpose of the experiment.

Results and conclusions. All the results are presented in Table I. In the first

TABLE I

SPONTANEOUS RECOVERY OF SALIVARY CONDITIONED RESPONSES OF 12 ADULT HUMAN *Ss* AFTER 48 HR., AS DEPENDENT UPON THE DEGREE OF EXTINCTION AND FORGETTING

Entries in first three rows represent means of net conditioned salivation of 2 *Ss* in mg. cf saliva per 1-min. periods. Entries in the next five rows are expressed as percentages of initial preextinction conditioned salivation. The last three rows present recovery as a percentage of extinction.

Experimental conditions	Extinction stages (number of extinction trials before recovery)					
	4	8	12	16	20	24
Magnitudes of initial CRs	316	272	295	236	250	218
Magnitudes of extinguished CRs	231	160	162	109	98	72
Magnitudes of recovered CRs	248	201	211	161	164	137
Extinguished CRs as percentages of initial CRs	73	59	55	46	39	33
Percentage of extinction	27	41	45	54	61	67
Recovered CRs as percentages of initial CRs	78	74	72	69	66	63
Percentage recovery when no forgetting is assumed	5	15	17	23	27	30
Percentage recovery when 14% forgetting is assumed	15.2	23.3	24.7	29.4	32.5	34.6
Recovery as a percentage of extinction when no forgetting is assumed	17.8	36.6	38.0	42.6	44.3	44.7
Recovery as a percentage of extinction when 14% forgetting is assumed	56.3	56.7	54.9	54.5	53.2	51.7
Recovery as a percentage of extinction when an additional 5% of forgetting of recovery itself is assumed	57.1	57.9	56.1	56.0	54.8	53.4

three rows of the table are given the magnitudes of the initial, extinguished, and recovered CRs in mg. of conditioned salivation per 1-min. periods, every entry being a mean of 2 *Ss*. The next three rows show the extinguished CR, the recovered CR, and the amount of extinction as percentages of the values of the initial CR. Then, come two rows in which recovery is presented both when no forgetting of the non-extinguished portion of the CR is assumed and when 14% of such forgetting is postulated. Finally, the last three rows sum up in a way the entire findings of the experiment in revealing recovery as a percentage of extinction in accordance with the three hypotheses discussed in the beginning of the article; namely, (1) when no

forgetting of the non-extinguished portions of the CRs is assumed, (2) when such forgetting (14%) is posited, and (3) when an additional forgetting (5%) of the recovery itself is further supposed.

Examining the results we see rather clearly that the hypothesis that spontaneous recovery is solely a function of extinction is much less satisfactory than the one in which a factor of forgetting is also assumed. In the first case when no forgetting is postulated, the dependence of recovery upon extinction is extremely irregular, ranging from 17.8% to 44.7%; where a forgetting of 14% is postulated, the relation becomes nearly linear, varying only from 51.7% to 56.7%. (Even greater linearity is attained when some additional forgetting of the recovery itself is postulated, but the difference is small.) We may, therefore, conclude: (a) that the amount of spontaneous recovery of a partly extinguished CR is a composite function of the amount of extinction and of the amount of forgetting of the non-extinguished portion of the CR; and (b) that the slope of recovery is much steeper than that of forgetting. After 48 hr. the amount of forgetting was 14% and of recovery 53%-57%.[2]

[2] It is interesting to treat in this manner the results of C. I. Hovland (*J. Exper. Psychol.*, 21, 1937, 52), whose study is the only one that is in any way comparable with that of the writer. Hovland found that, after 24 hr. a CR which had been extinguished to 65.1% of its normal value recovered 12.6%, while a CR of 90.5% of its initial effectiveness lost 7.8%. This apparent contradiction is readily resolved by assuming a factor of forgetting and solving the simultaneous equations of $9.5x - 90.5y = -7.8$, and $34.9x - 65.1y = 12.6$; where x and y are respectively the rates of recovery and forgetting. The obtained values are $x = 0.64.6$ and $y = 0.15.4$; which are very much in line with what is known about the rate of forgetting on non-extinguished CRs and the rate of recovery of CRs that had been completely extinguished.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

LXXXVIII. Acquisition by Children of the Cause-Effect Relationship

By John Irving Lacey and Karl M. Dallenbach

In 1926 there was reported the first of a series of studies of children's learning of logical relationships.[1] In that study the relationship of opposition was investigated with two children. Neither of the two understood the meaning of opposition at the beginning of the test, but the older child, a 6 yr. old boy, learned it immediately upon gaining insight into the nature of the test problem. The younger child, a 3 yr. old girl, was, however, unable to learn it despite coaching with numerous illustrative examples. Thus age seemed to be a factor in the learning, and learning seemed to be attained suddenly and not in the gradual manner required by the trial-and-error theories.

Because of the theoretical significance of these results, the study was extended in 1929 to 100 school children whose ages ranged from 5 to 7½ yr.[2] The new results confirmed the anticipations of the earlier study. Age was found to be a condition and learning was found to come suddenly with the acquisition of insight. Children of the social status of the group under investigation know the meaning of opposition without special instruction by the end of the eighth or the beginning of their ninth year and in the age-group 5·3-5·9 there is a probability of 0.5 of learning the relation after a small amount of instruction.

Recently, Schooley and Hartmann broadened the scope of the study to include the relationships of whole-part, part-whole, action-agent, agent-action, attribute-substance, substance-attribute, genus-species, and species-genus.[3] They followed the method adopted in the second study, except that they decreased the number of test-words from 25 to 10. The conclusion they reached was that the learning of standard logical relationships is not a slow, gradual, trial-and-error process, but rather a sudden and insightful process, giving evidence of permanent mastery. Their results indicate, for children of the social status of their *Ss*, a probability of 0.5 of learning the relationships that they studied at the age-levels 5·1-5·7 to 6·4-6·10.

The purpose of the present study is to carry the investigation to the relation of cause-effect.

Method

Selection of test-words. The cause-effect relationship is difficult both for the children who give the replies and for the experimenter who has to decide whether

* Accepted for publication February 15, 1938. The experiments were conducted in the Ithaca public schools. We acknowledge our indebtedness to Mr. C. L. Kulp, Superintendent of the Ithaca Schools, and to the teachers who assisted us in this study.

[1] K. M. Dallenbach, A note on the immediacy of understanding a relation, *Psychol. Forsch.*, 7, 1926, 268 f.

[2] George Kreezer and K. M. Dallenbach, Learning the relation of opposition, this Journal, 41, 1929, 432-441.

[3] Mary Schooley and G. W. Hartmann, The rôle of insight in the learning of logical relations, this Journal, 49, 1937, 287-292.

the replies, frequently ambiguous, are adequate. Much depends upon the test-words. We, therefore, determined empirically, in a preliminary experiment, what words were meaningful in a cause-effect context for children of ages 5 to 7½ yr. We selected the words from Thorndike's list.[4] Our two criteria of selection were: (1) that the words fit easily into a question; and (2) that they be 'heavily weighted' by Thorndike. The words selected were then put into simple questions to which the answer was a categorical 'yes' or 'no.' Sample questions are:

(1) Would you *burn* yourself if you put your hand in a fire?
(10) Would you be *thirsty* if you did not drink anything?
(20) Would *night* come if the sun stayed in the sky?

Four-page booklets, convenient for handling, were prepared with numbered lines containing the words "yes" and "no." The *S*s were instructed to listen carefully to the questions, read one at a time, and to underline the appropriate word, "yes" or "no." They were also cautioned not to whisper, answer aloud, or copy from their neighbors. On the whole, excellent coöperation was secured.

This preliminary investigation covered pupils in three class-rooms: one kindergarten and two first-grade. In all, we secured 94 papers which we could use. We scored the papers, summed the number of times every sentence was answered correctly, and translated the sums into percentages. When the rank-order coefficient of correlation between the percentages for the words and the Thorndike weighting was calculated and translated into a product-moment coefficient,[5] we found that $r = +0.06$, P.E., 0.12.

The lack of covariability between our results and Thorndike's weights can be explained in several ways. The possibility that directly concerned us, however, was that our sampling had been poor. To check this possibility, the papers were divided at random into halves and the rank-order coefficient between the halves was calculated, and translated, as before, into a product-moment coefficient. We found $r = +0.86$, P.E., 0.06, which indicates that our sampling was reasonably reliable. We, therefore, selected as test-words for the main experiment, the words (see Section E under Procedure) in those sentences which had been most often answered correctly.

Subjects. Age and school attendance were the only criteria in the selection of the *S*s. Since the work of Schooley and Hartmann demonstrated no reliable sex difference, we paid no attention to that factor. Twenty children were chosen for every one of the following age-groups: 5·0-5·6 (Group I); 5·7-5·12 (Group II); 6·0-6·6 (Group III); 6·7-6·12 (Group IV); 7·0-7·6 (Group V); 7·7-7·12 (Group VI); 8·0-8·6 (Group VII); 8·7-8·12 (Group VIII).

Procedure. Our procedure was comparable in all respects with that of Schooley and Hartmann. When *S* was brought to the experimental room, he was first put at his ease. Then he was asked:

(A) "Would you like to play a game of 'cause-effect' with me this morning?" After securing the child's assent, *E* asked, "Do you know what cause and effect mean?"
(B) To a *negative* answer, *E* said, "Yes you do. Tell me what is the cause of *sleep*." To an *affirmative* answer, *E* said, "All right, tell me what is the cause of *sleep*."
(C) To an *incorrect* answer in (B), *E* said, "No, you are wrong; that is not the cause of sleep. Now tell me what is the cause of a *blister*."

[4] E. L. Thorndike, *The Teacher's Word Book*, 1921, 1-134.
[5] H. E. Garrett, *Statistics in Psychology and Education*, 1925, 192.

To a *correct* answer, *E* said, "That's right; now tell me the cause of a *blister*."

(D) To an *incorrect* answer in (C), *E* said, "No, you are wrong. Now listen. People *sleep* because they are tired; people get *blisters* because they burn themselves. The cause of sleep, then, is being tired; the cause of a blister is a burn. Do you understand now what cause and effect mean?"

To a *correct* answer, *E* said, "That's right," and then repeated the child's answers and concluded with "Do you understand now what cause and effect mean?"

(E) To a *negative* answer in (D), *E* said, "Well, I'll see if you do. Tell me what is the cause of (1) *bleeding*—(2) *smile*, (3) *smoke*, (4) *being hungry*, (5) *melting*, (6) *eating*, (7) *drinking*, (8) *sickness*, (9) *punishment*, (10) *being thirsty?*"

To an *affirmative* answer, *E* said, "All right, I'll see if you do. Tell me what is the cause of *bleeding*," etc.

(F) As *S* returned his answers, *E* simply replied "right" or "wrong." If *S* did not show, by his answers to the first 6 words of the test list, that he understood the relationship, *E* again told him the correct answer to *sleep* and to *blister* and then repeated the first six words, saying "right" when a correct answer was returned and supplying a correct answer when an error was made. The last four words were given without correction or comment.

S was credited with having learned the cause-effect relationship if he replied correctly to any one of the first six words of the test-list and to all the remaining words. We did not demand scientifically accurate causes, but merely common-sense explanations which would indicate that the relationship was understood. An 'after-this-therefore-because-of-this' explanation sufficed.

Results

(1) Age at which the relation is understood. Table I analyzes and summarizes the results of the experiment. The first four sections of our procedure were designed to elicit the following information: (1) Does the child affirm that he understands the meaning of cause and effect when those terms are used alone? (Section A); (2) Does the child understand the meaning of the terms when used in specific questions? (Sections B and C); (3) Does the child affirm an understanding after this instruction? (Section D).

To the question in Section A ("Do you know what cause and effect mean?"), only 2 out of 160 *Ss* answered "yes." Yet in Sections B and C 67 were able to give correct answers to specific questions. Of these, 35 answered correctly in both sections, 7 in B alone, and 25 in C alone. To the question in Section D ("Do you understand now what cause and effect mean?"—practically a repetition of the question in Section A) 156 *Ss* answered "yes;" and though many (84) demonstrated later in Sections E and F that they did understand, almost as many (76) failed, according to our criteria, to do so.

Table I shows, in Sections B and C, that knowledge of the cause-effect relationship is correlated with age. The younger children failed in these preparatory sections of the experiment; the older children, as their age-level advanced, more and more demonstrated knowledge though they had just disclaimed it in Section A. The result, that children in general understand the meaning of cause and effect by the end of their eighth or the beginning of their ninth year, corroborates those obtained by the earlier experimenters with other relations.[6]

(2) Age at which the relation is learned. Sections E and F in Table I show the

[6] Kreezer and Dallenbach, *op. cit.*, 434 f.; Schooley and Hartmann, *op. cit.*, 290.

number of *Ss* in the different age-groups who demonstrated that they had, according to our criterion, learned this relationship. The percentage of success varies with age. Since the frequencies follow in general the distribution of an ogive curve—the inversion of the first-order at Age-Group VI is probably an error of sampling—an age-limen was calculated by the method of constant stimuli,[7] as in the earlier studies.[8] The value obtained was $L = 6.81$ $(h = 0.25)$.

TABLE I

COMPREHENSION OF CAUSE AND EFFECT AT DIFFERENT AGES AND IN DIFFERENT SECTIONS OF THE EXPERIMENT

Section	Answer	Age-Group*								
		I	II	III	IV	V	VI	VII	VIII	Totals
A	Yes	0	0	1	0	1	0	0	0	2
	No	20	20	19	20	19	20	20	20	158
B and C	Both correct	1	3	1	3	4	6	8	9	35
B alone	Correct	0	0	1	0	1	2	3	0	7
C alone	Correct	1	3	0	4	4	3	5	5	25
D	Yes	20	19	20	20	18	20	19	20	156
	No	0	1	0	0	2	0	1	0	4
E	Correct	3	7	5	9	7	9	13	17	70
F	Correct	1	1	5	2	5	0	0	0	14
	Correct: No.	4	8	10	11	12	9	13	17	84
	%	20%	40%	50%	55%	60%	45%	65%	85%	
E and F	Failures:									
	Instable	15	9	9	9	8	11	7	3	71
	Complete	1	3	1	0	0	0	0	0	5
	Total	16	12	10	9	8	11	7	3	76

* I=5 yr. 0 mo. to 5 yr. 6 mo.; II=5·7–5·12; III=6·0–6·6; IV=6·7–6·12; V=7·0–7·6; VI=7·7–7·12; VII=8·0–8·6; VIII=8·7–8·12.

Our age-groups, it will be recalled, have 6-mo. ranges. Since the group-median was used in the calculations, the value for L must also be construed as a median and the age-group to which it corresponds should be considered as extending from 6.56-7.06 yr. These computations indicate, therefore, that children of the 6·7-6·12 age-group have, under the conditions of this study, a probability of 0.5 of learning the cause-effect relationship.

Of the logical relations thus far considered, the age-limen for cause-effect is the highest (see the accompanying summary). The spread of the various age-limens is not wide; it extends over a period of only 17 mo., from 5·4 to 6·9. The narrow range

Relationship	Age-limen (yr. & mo.)	Relationship	Age-limen (yr. & mo.)
action-agent	5·4	genus-species	5·11
agent-action	5·4	whole-part	5·11
opposite	5·6	part-whole	6·1
attribute-substance	5·10	species-genus	6·7
substance-attribute	5·11	cause-effect	6·9

[7] Cf. E. G. Boring, Urban's tables and the method of constant stimuli, this JOURNAL, 28, 1917, 280-293.

[8] Kreezer and Dallenbach, *op. cit.*, 439; Schooley and Hartmann, *op. cit.*, 290 f.

of these limens suggests that we are dealing with a capacity or an ability that depends upon maturation. The failure of the younger children to learn the meaning of the various relations studied supports that view. Despite numerous repetitions of illustrative examples, the younger children simply had not matured sufficiently to learn the relational significance.

(3) Manner in which the relation was learned. All the *Ss* credited with learning the relation—E and F in Table I—learned it suddenly or insightfully. From associations whose determinations were unknown and incorrect within the meaning of our experiment, their answers shifted abruptly to associations determined by their comprehension of the causal relation. Their replies to one word were incorrect but to the next and to all subsequent words were correct. The learning curves dropped suddenly from 100% to 0% failures. From the moment that the children gained insight, their answers were correct and in general quickly and confidently given.

The *Ss* failing to learn divide into two distinct groups (see Table I). Some of them (5 in number, all from the lower age levels) failed completely. These failures are like those reported in the earlier studies: the replies to all the test-words were incorrect. The illustrative examples did not give those *Ss* insight into the problem and they did not learn. Other *Ss* (71 in all and distributed in decreasing numbers from the lowest to the highest age-groups) were marked as 'failures,' in accordance with our criteria of learning, because their correct and incorrect replies were interspersed. Their comprehension was apparently 'instable.' These *Ss* indicated that they had learned the relation at one test-word only to demonstrate at the next, by missing the relation entirely, that they had not. For example, *S* 8 (Age-Group VIII) replied to *drinking*, the seventh test-word, with "because you're thirsty"—an adequate cause-effect relation; his reply to *sickness*, the eighth test-word, was "you are ill"—a relation of similarity. Nothing comparable to this type of failure was obtained in any of the earlier studies. In the earlier studies, no *S* failed after a success; once a correct answer, always a correct answer. In the present study, however, a correct answer to one word was no guarantee that correct answers would be received to the following test-words.

Figs. 1 and 2 present graphically the results in Sections B, C, E, and F of our procedure for Age-Groups I and VIII—our extremes. The *Ss* are listed by number along the ordinates: the various sections of the procedure, and the test-words in Sections E and F, are arranged along the abscissæ. Black rectangles indicate success, open rectangles failure, and blank spaces omissions required by our procedure. These figures demonstrate clearly what happened, in varying proportions, in all the age-groups. The 'instable' type of performance is illustrated by the records of 15 *Ss* in Age-Group I and by *Ss* 2, 5, and 8 in Age-Group VIII.

The 'instable' records may be a function not of learning but of the test-words employed. Though we sought, by means of preliminary experiments with kindergarten and first-grade pupils, to select test-words that were comparable in difficulty, it may well be that all of our words did not demand the same degree of knowledge. If one word was 'easy' and the next 'hard,' the instable records are understandable. Since the experimental procedure did not permit us to go back of the *Ss*' replies, we have no way of knowing whether this explanation is valid. It is obvious, however, that *S* may fail in a particular test-word, not because he has failed to learn the causal relationship but because (1) he does not know an adequate or sufficient cause,

(2) he lacks the necessary vocabulary,[9] (3) his intended meaning is not understood by *E*, and (4) he is distracted by remote causes. As an example of (3): *S* 1 (Age-Group I) replied to *bleeding* with "beets." By some context peculiar to the child,

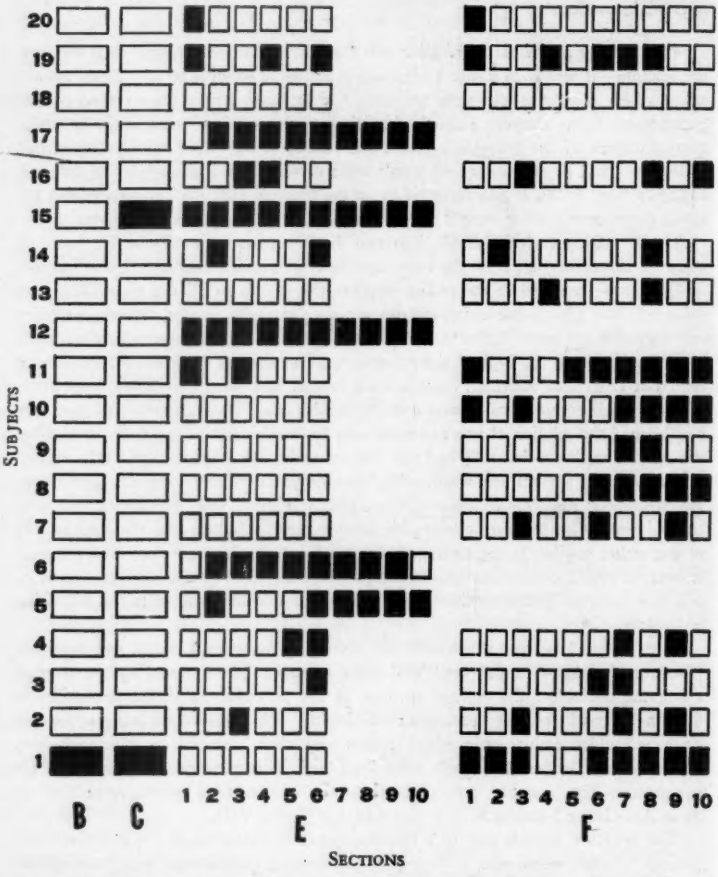

FIG. 1. COMPREHENSION OF CAUSE AND EFFECT IN AGE-GROUP I (5·0-5·6) IN DIFFERENT SECTIONS OF THE EXPERIMENT
(Black rectangles designate correct answers; open rectangles, incorrect answers.)

[9] Victoria Hazlitt (Children's thinking, *Brit. J. Psychol.*, 20, 1930, 354-361) shows that children may understand a relationship that they are unable to verbalize.

that might be a correct answer, although it was considered wrong by *E*. Even more extreme instances of this sort may have occurred, but they of course could not, under the procedure of the present study, be detected by *E*. As an example of (4): thirst,

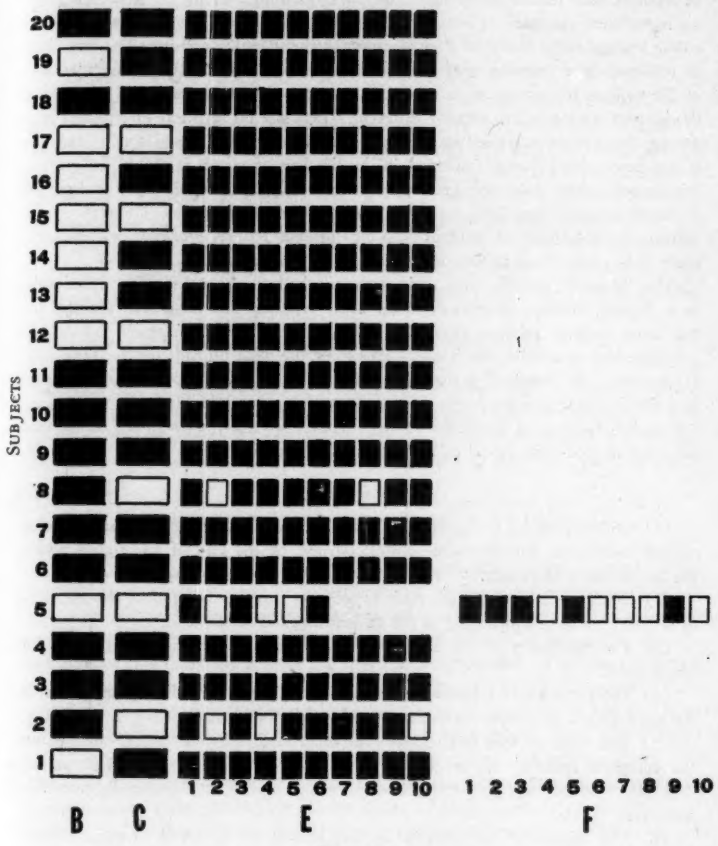

FIG. 2. RECORDS FOR AGE-GROUP VIII (8·7-8·12)

as the cause of *drinking*, may be known by *S* but he may nevertheless reply, "I do not know," because he does not know what is the cause of thirst. Unless, therefore, test-words are chosen which do not yield these incorrect and ambiguous replies, or a procedure is devised which either avoids or detects them, we cannot be sure that the results are not in part at least, a function of the test-words employed.

Which, then, of the 'instable' *Ss*, if any, should be credited with learning the relation? Or should all of their records be regarded as evidence of trial-and-error learning? It is true that in this study, though not in the earlier ones, the criterion of learning was 'loaded' in favor of insightful learning; all the *Ss* who failed after a success were classified as not having learned the relation. In other words, the *Ss* either learned insightfully or they were not credited with learning. The hypothesis of trial-and-error learning does not, however, resolve the difficulty. For what item in the various test-situations is repeated or exercised? The content and meaning of the various situations are entirely different. There are no identical or constant items among them to be exercised or 'stamped in.' Every correct reply is new and after it has been given, though 'stamped in' by the law of effect, it would be incorrect for the subsequent test-word. Frequency will not help in the explanation and recency, if effective, would inevitably lead to a continuous series of errors. These theories of learning cannot, therefore, explain even the 'instable' reports, which at first inspection seem to be particularly appropriate examples of trial-and-error.[20]

The 'instable' records, and their ambiguity, place the cause-effect relationship in a slightly different category from the other relations that have been studied. It is the most difficult relation from the point of view of experimental control and interpretation of results, and it also seems to be the most difficult for the *Ss* to learn. If, however, the 'instable' performances are to be regarded as instances of learning and the lapses explained as we have suggested above, then, since there were only 5 complete failures—a decidedly smaller number, relatively, than in any of the other relations studied—the causal relation is the easiest of all.

Summary

(1) Children of the social status of our *Ss* learn the cause-effect relation without special instruction during middle childhood, *i.e.* by the end of the eighth year or the beginning of the ninth.

(2) With special instruction, such as we gave our *Ss*, children of the age-group of 6·7-6·12 have a probability of 0.5 of learning the relation.

(3) The age-limen of the cause-effect relation is the highest of the relations investigated.

(4) The cause-effect relation, when learned, is acquired suddenly and thereafter the answers are in general given more quickly and confidently.

(5) The children who fail to learn the relation divide into two distinct groups: the complete failures, whose replies to all the test-words are incorrect; and the 'instable,' who fail only part of the time and whose replies are haphazardly correct and incorrect.

(6) The failures of the 'instable' *Ss* may be due not to a lack of comprehension of the relation but (1) to a lack of specific knowledge, (2) to a lack of vocabulary, (3) to the experimenter's lack of understanding of *S*'s meaning, and (4) to distraction by remote causes.

(7) The trial-and-error theories of learning are inadequate, being unable to account for the instances of insightful learning nor for the performance of the 'instable' *Ss*.

[20] For a discussion of the inadequacy of the trial-and-error theories to children's learning of relations, see Kreezer and Dallenbach, *op. cit.*, 436 ff.

APPARATUS

THE TYPEWRITER AS AN AID IN ITEM ANALYSIS

By SOLOMON DIAMOND, New York, N.Y.

There are many psychological investigations, particularly those in which questionaries are used or rankings made, in which the data for each subject consists of a series of readily symbolized responses, which must not only be scored for the individual but also treated statistically item by item. The best aid for such analysis is of course a card-sorting machine, but this is a luxury which few can afford. I have made very good use of the typewriter to compile and analyze such data. The data is recorded in the speediest possible manner in the most compact form possible, and one which permits considerable economy of effort in analysis. Others, who had been using much more cumbersome procedures, have found the method which I shall describe helpful to them as well.

Data sheet. The record of every individual *S* is transferred to a data sheet as a single line of type, without skipping any spaces between items. The way in which this is done will be readily apparent from an illustration, which I take from a data sheet used in a previously published study. The record shows the age and sex of every *S*, with answers to a 48-item questionary, in which the *S*s indicated that statements were right or wrong as applied to themselves, or that they were uncertain.

```
18f rrwwwrrrwrrwwwwr?rrwwrwrrwwrrwwrrwrrwwwwrrwww
20m wwwrrrrwwwwwwwwrrwrwrr?rrr?rww?wrrrrwr??wwwrw?rw
20f rrwrw?rrwwrw?wrr?wrwrr?r?rrrr?wrrrwr?www?wwrrr?w
```

It is clear that any data which are subject to statistical analysis can be presented in this way. Marital status might have been shown in an additional column, or by using small and capital letters to indicate sex. Educational status might be shown in a column in which a numeral would indicate an elementary school grade, and the small letters f, i, j, s, would indicate the freshman, intermediate (sophomore), junior and senior years of high school, while the same letters capitalized would indicate these years in college, and other letters could be introduced for graduate and professional training. Occupational level could be shown in another column, and use of small and capital letters in this column might indicate employment and unemployment.

When the data for a group of *S*s has thus been placed on one sheet of paper, it presents a mass of symbols which is utter chaos when it is all exposed to the eye. While scoring, or doing statistical analysis, only one or two rows or columns should be exposed at a time.

Scoring key. The scoring key used will vary in design according to whether separate totals for odd and even items are wanted, whether the items are to be given different weights, etc. The key used in connection with the records illustrated was made as follows. On a sheet of paper, the series of correct answers was typed, for odd items only, properly spaced. Two lines below, the correct answers for even items were typed, to fall under the intervening spaces in the first row. The sheet was taken from the typewriter, and a slot cut in it, between these two rows of

letters (see Fig. 1). This key was then placed on the data sheet, to expose one row of type at a time. (The blank spaces correspond to items which were not scored, because they lacked differentiating power.)

If items are weighted differently, each row on the key may give the weights for a certain type of answer.

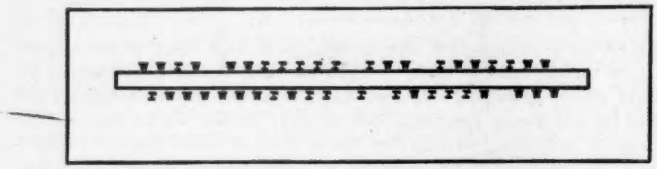

FIG. 1. SCORING KEY FOR TYPEWRITER ANALYSIS

Item analysis. For item analysis, columns are exposed one or two at a time. A device which assists in this is made from a celluloid strip 14/10 in. wide. (If élite type is used, it must be 14/12 in. wide.) Three slots, each 1/10 in. wide, and long enough to expose a full column, are cut in this strip. The first of these slots is cut 3/10 in. from one edge of the strip, the second is cut 2/10 in. from the first, and the third is cut 1/10 in. from the second, leaving 5/10 in. to the other edge of the strip. If the narrow strips of celluloid between the slots tend to twist or buckle, or if they have been broken in the cutting, the whole can be made quite strong by fastening some bands of transparent Scotch tape around the strip. With this device, one can expose any two columns which are separated by any number of intervening columns from one to eight. When the columns to be compared are farther apart than this, it is well to fold the sheet so as to bring them closer together, for ease in scanning. In this way, contingency tables may readily be constructed. (The one or two unused slots are covered by strips of heavy paper, whose ends are tucked into cuts made at the ends of the celluloid strip.)

Adaptations of this general method will suggest themselves to those working with slightly different types of data. In studying the relation of a vote on one question to a vote on another question, in a 10-item referendum, I found it expedient to record the votes on each ballot with a dot to represent 'yes,' and simply skipping a space for 'no.' The record of a few ballots appears as follows:

Again, let us assume that we are working with rankings made by a group of *Ss* for 12 persons or things. To avoid the waste of time and effort involved in keeping columns in the usual way, we need one-place symbols for the numbers ten, eleven, and twelve. We decide that the zero shall represent 'ten,' and that "q" and "w," the first two letters in the bank of keys beneath the digits, shall represent 'eleven' and 'twelve.' The rankings made by several *Ss* would then appear on our data sheet as follows:

458903762wq1
029q746851w3
w541q6293078
6702813wq549

In treating the data, we expose one column at a time, and record the frequency of each ranking within it. The fact that letter-symbols have been used for some of them does not give rise to any inconvenience.

AN INEXPENSIVE, VARIABLE MULTIPLE-CHOICE MAZE

By ARTHUR BERMAN, University of California

Many mazes, adapted for human subjects, have been described in the psychological literature.[1] We add another to the long list because the maze we have devised is inexpensive and simple in construction and operation.

Our maze consists of a panel mounted on four uprights fitted together to make an inverted box. The panel, 12 x 12 in. square and 7/8 in. thick, is made of mahogany, hence holes may be drilled in it without splintering. The holes, 19 rows of 19 each (361 in all) are 1/8 in. in diam. and 3/8 in. apart (see Fig. 1). The panel is well sanded and shellacked so one hole cannot be distinguished from another; thus uncontrollable reaction-cues are eliminated. Beneath the panel, a metal plate is slid in slots like a drawer. Between the panel and this plate a reaction-pattern is placed. This pattern may be made of paper or of any non-conducting electrical material into which holes may easily be cut—we, however, used fiber board. The holes in this pattern may follow any design the experimenter wishes and as many different patterns may be made as desired.

At the center-head of the panel (see Fig. 1), a small 4–6 volt lamp is set which operates on three dry cells. One lead from the lamp goes to the positive pole of the battery and the other to the metal plate. A stylus, connected to the negative pole of the battery, is inserted in the panel-holes. When it touches the metal plate beneath, the electrical circuit is completed and the signal light flashes on. The flashes may be taken as indications of success or failure accordingly as the holes are cut in the reaction-pattern. A buzzer, an electrical shock, or other signal devices may quickly and easily be substituted for the electric light.

This maze is, as we believe, an improvement upon the earlier ones described. It has greater range of variability than any of them (361 holes and thus that many choices); it has no bolt heads to become scratched, and no complicated electrical circuits to wire; it is inexpensively made, and it is maximally variable as diagonal

[1] Shirley Morrison and T. W. Cook, An adjustable maze for visual and non-visual learning, this JOURNAL, 46, 1934, 141; R. G. Whisler, A universal flexible elevated finger maze, *J. Gen. Psychol.*, 10, 1934, 482-484; J. Buel, A simple, inexpensive finger maze for experimental and classroom use, *ibid.*, 17, 1937, 192; J. R. Knott and W. R. Miles, Notes on the history and contribution of the stylus maze, *Ped. Sem.*, 35, 1928, 415-427; Herbert Gurnee, A portable bolthead maze, this JOURNAL, 51, 1938, 405-406.

and rectilinear paths may be followed and new problem-patterns and goals quickly substituted. The last consideration is highly desirable in experiments which attempt to measure the comparative value of various rewards, the effect of an old goal on a new one, and the effect of changes in level of aspiration or satiation.

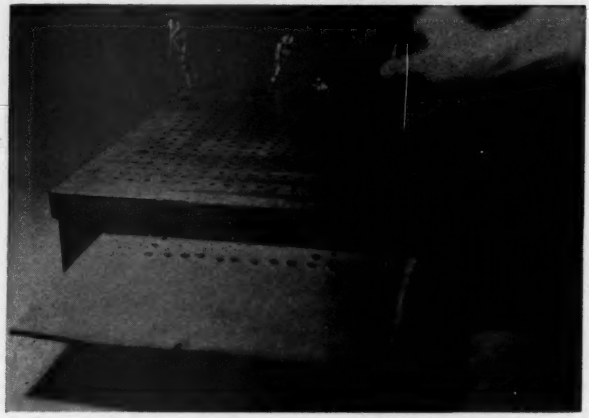

FIG. 1. THE VARIABLE MULTIPLE-CHOICE MAZE

The small cost and facility with which this maze may be built makes it an attractive apparatus where experimentation in small groups is desired. Throughout a period of three months it has been given a rigorous test in the laboratory of the University of Southern California.

We have found it desirable to use substances for the reaction-pattern as similar to the metal plate as possible. The subject is then unaware from secondary cues when he strikes the pattern or the metal plate. This, however, is of no great importance since the cue is not available until the stylus is inserted and is thus not retroactive. Once the stylus is inserted in a hole, it is counted a trial, and what follows is in this instance not of great significance.

In the final analysis, the chief advantage of the present maze lies in its inexpensiveness and great adaptibility.

A DEVICE FOR OBTAINING CONSTANT OLFACTORY STIMULATION

By A. L. WINSOR and B. KORCHIN, Cornell University

In our studies on the parotid secretory accompaniments of olfactory adaptation we found it necessary to devise some means of obtaining a stimulation which remained constant in intensity throughout the experimental period. For this purpose the

apparatus described here served very satisfactorily. It can be constructed with relative ease and little expense.

An oval shaped opening (5 mm. x 20 mm.) is blown out of the wall of a piece of glass tubing (8 mm. bore; 15 cm. length). To prevent the liquids that are to pass through the tube from overflowing the edges of the opening are raised somewhat above the level of the rest of the tube. If it is desired to mix the stimulating vapors with air or other vapors, two small tubes can be attached by adhesive tape in the manner illustrated in Fig. 1. Fig. 1 gives in outline a diagram of the apparatus

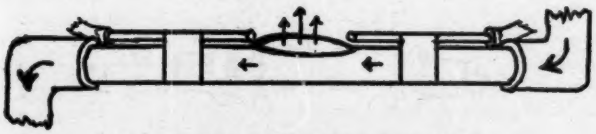

FIG. 1. DIAGRAM OF OLFACTORY STIMULATOR

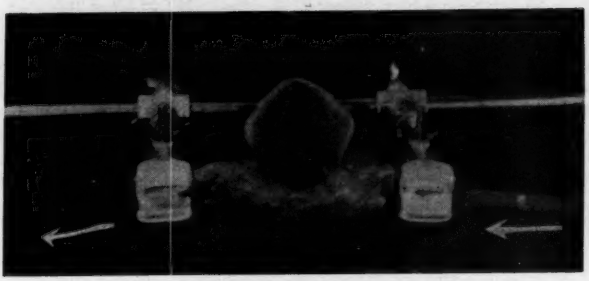

FIG. 2. THE OLFACTORY STIMULATOR
(Nasal shield in place.)

before the dental wax is molded around it. Two or three cakes of dental impression compound are sufficient to cover the tubes and be molded into the shape of the nose-piece shown in Fig. 2. By applying a heated cloth to the upper portion of the nose-piece it can be easily softened and molded to fit snugly around the *S*'s nose. The dental impression compound is a thermoplastic wax which is rigid at body temperature, but which becomes soft and pliable at about 40°C.[1] The wax should be heated in a waterbath. The nose-piece is clamped to a stand and adjusted to the convenience of *S*, whose head is held in place by means of a headrest.

The liquids that are to flow through the tube are kept in bottles on a shelf above *S*'s head. The rate of flow can be adjusted by means of stop-cocks. Rate of flow can also be kept constant by means of Mariotte's bottles or other mechanical means.

[1] For a similar use of the dental impression compound see H. Abrahams, D. Krakauer, and K. M. Dallenbach, Gustatory adaptation to salt, this JOURNAL, 49, 1937, 463.

THE MANNING PNEUMOGRAPH

By David Kirby Spelt, University of Mississippi

A simple pneumograph has been recently devised by Dr. I. H. Manning, Professor of Physiology, Emeritus, in the Medical School of the University of North Carolina. It is especially valuable because it is inexpensive to make and can be used satisfactorily with subjects of any size from rabbits up to human beings, without changes in dimensions.

Fig. 1. The Manning Pneumograph

FRT = Flexible rubber tube
SS = Solid rubber stopper
DS = Drilled rubber stopper
MR = Metal ring
MT = Metal tube
H = Hook

It consists essentially of a flexible rubber tube, approximately 11 in. long and ⅞ in. in diameter.[1] Deeply corrugated transversely, it stretches longitudinally. One end is completely closed by a solid rubber stopper into which a small hook is securely fastened. In the other is a drilled rubber stopper, from which projects a piece of metal tubing of any convenient bore. The rubber stoppers are held in place by friction. A length of cloth tape or of chain is fastened permanently to a small ring soldered to the metal tubing, not too close to the free end. In use, this passes around the subject's back to the hook at the other end of the pneumograph, and a rubber hose, slipped over the metal tube, connects with a recording device.

The instrument is adequate both for research purposes and for general laboratory work. Because the rubber is not under tension its life is long if it is kept away from heat and strong light when not in use.

[1] This tube can be purchased from Phipps & Bird, Inc., 915 E. Cary St., Richmond, Va.

NOTES AND DISCUSSIONS

Binocular Fusion and the Locus of 'Yellow'

In a note (A binocular color phenomenon, this JOURNAL, 51, 1938, 769 f.), Dr. E. G. Bugg reported the observation of a violet after-image following the use of Hecht's Binocular Color Mixer to secure a 'fusion' yellow from monocular red and green. This violet after-image he assumes represents also a fusion product on a higher brain level. Since Dr. Bugg's discussion conveys an impression of the derivative character of 'yellow' and its cerebral rather than retinal locus, it seems desirable to amplify an earlier criticism of the technique employed[1] and the fallacies involved.[2] Binocular fusion or summation may be readily enough conceded, superseding in certain cases retinal rivalry. When, however, such fusion is invoked as generating so unique a quality as yellow, from two other as unique or primary ones—in this case red and green—caution demands a careful scrutiny of the conditions.

In 1928, Hecht (whose method was followed in the above study), after reporting the difficulties and hesitations of his predecessors of the last quarter-century in the field of binocular fusion, cannily sidestepped the pitfalls of earlier observers. First, as in many other Helmholtzian or tri-stimulus observations, the dice were weighted by passing over the standard 'red' and 'green' filters of the Wratten 'monochromat' series, and picking instead two wide-band yellow-green and yellowish red ones (no. 58 and 29), designed primarily for photography.[3] Secondly, a light source not of the low wattage for which the filters were designed but of 100 to 250 watts was chosen. Thirdly, the set-up was so arranged that the lens focus favors 'yellow'

[1] S. Hecht, On the binocular fusion of colors and its relation to theories of color vision, *Proc. Nat. Acad. Sci.*, 14, 1928, 237-241. For criticism of the same, see E. Murray, Color problems: the divergent outlook of physicist and psychologist, this JOURNAL, 32, 1930, 124, footnote 20.

[2] This critique is based upon experimental observations made in the Cornell Psychological Laboratory, in a special optical unit outfitted in part by a grant-in-aid of 1930-1931 from the National Research Council secured through the coöperation of Professor Madison Bentley.

[3] The three wave lengths of the I.C.I. tri-stimulus colorimetry tables of 1931, according to D. B. Judd of the Bureau of Standards, are 700, 546, and 435.8 mμ. In a spectrum of average intensity, these three stimuli yield a deep red verging on crimson, a yellow-green, and violet. Helmholtzian experimenters have a disconcerting way, however, of shifting the 'red' stimulus (700) about in the long-wave half of the spectrum, 'for convenience's sake.' Von Kries even pushed it in as far as 589 mμ, in order to line up his curves for dichromats satisfactorily; while Hecht uses 650 or 570 indifferently for the same purpose, substituting also 458.7 for 440 as the occasion demands.

As for the mid-spectrum stimulus (546), this is centered far from the 505 strip which is achromatic in red-green blinds, and which, owing to its invariability of hue with altered intensities, tends today to be picked by all theorists as the best excitant for the 'green' receptors. As a matter-of-fact, the mid-spectrum region of 525 to 545 mμ, favored by old-school Helmholtzians in complete disregard of peripheral and color-blindness data, is so yellow in tone that in 1922 Dunlap proposed designating it as *chlor* rather than green.

There is apparently a vague assumption that the optimal wave length may fail to yield a high-chroma primary owing to the overlapping of the curves of the other two primaries. Yet as early as 1892, König and Dieterici shifted their *Grundempfindungen* to purplish red, 505 and 470 mμ, to fit the mixture data of color-blind.

radiations, and the fusion area falls within the yellow-enhancing region of the retina, the macula lutea. Finally, as often happens in physical experiments, the rapidity of color adaptation or fatigue, especially for weak reds and greens, and the consequent need of brief exposures, was ignored, and no time limit set on an observation.

Checking over this set-up a few years ago, with a view to repeating Hecht's observations on a mixed group, the writer speedily reached the conclusion that no mystical fusion of red and green excitations in the higher centers above the chiasma need be invoked, to account for the binocular yellow square.[4] The summation of the yellow valences of the two monocular stimuli (mediated in all likelihood by 'yellow' receptors in the retinae), plus the selective action of macular pigment on the yellow component and its reënforcement by lens focus and other factors, would suffice to mask or swamp the red and green components. The latter are already weakened by the high intensity of the light source chosen; for red and green hues are notoriously keyed with low intensity mechanisms which pass out of function with increased intensities, these hues shifting into yellow or blue (notably in the bright light spectrum).[5] Further, both the red and the green components tend to adapt out quickly when there is no control of exposure-times.

Since, therefore, all the conditions point to enhancement of a yellow tone, excitations for which are probably already present monocularly, binocular summation appears to account fully for the phenomenon of the central yellow field, dispensing with the need of positing a brain locus for the creation of the quality yellow.

This solution automatically resolves the puzzle of the locus of Dr. Bugg's violet after-image. The latter becomes merely the summation of the two monocular complementary after-images of the yellow components of the yellow-red and yellow-green squares, of presumably retinal locus. The complementary after-images of the red and green components are too weak to count. Incidentally, the fact that the preceding sensory fusion is not yellow but yellow-green is indicated by the report of a *violet* rather than an indigo-blue after-image.

The status of yellow[6] as a distinctive sensory datum or quality, with photo-receptors in the retina in its own right (or in conjunction with its complementary blue), and its own unique excitation process in the neural tissues of the eye, is thus hardly affected either by Hecht's so-called demonstration of binocular color fusion, or by Bugg's repetition and extension of Hecht's observations into the domain of the after-image.

The first choice in 1801 of Thomas Young, originator of the tri-chromatic theory, for his mid-spectrum primary,[7] yellow is slowly but surely regaining its original

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[4] The simulation of a new or unique quality by excitations fused above the receptor level is of course in itself no novelty in sense psychology. In 1907-1909 the writer used this principle to effect an economy in the diversity of receptors necessary to explain tactual and organic or somaesthetic perceptions. (Cf. this JOURNAL, 19, 1908, 289-344, esp. 337 ff.; and 20, 1909, 386-446, esp. 441 ff.) In the olfactory field also, some such synthetic principle plays apparently a significant and often baffling rôle.

[5] This hue shift may readily be demonstrated on the spectrometer by isolating a band centering as does Filter 58 on 545 mμ. Though 545 mμ appears yellowish green at a voltage of 60, at 105 it becomes greenish yellow. Filter 29 (orange) centers on 610-620 mμ, a band which in the spectrometer at 100 v. has its yellow tone heightened, its reddish component weakened.

[6] *I.e.* the hue located in the spectrum in the neighborhood of the sodium lines.

[7] Later abandoned for green (yellowish), through a misunderstanding of Wol-

lead in three- no less than in four-component theorizing. In view of the steadily accumulating evidence of the prevalence of this quality in the peripheral field, in the visual world of the color-blind, and in the bright-light spectrum this is not surprising.

Nor does the claim to sensory primacy of this hue need to lean at all on the antiquated doctrine of 'purity' or unanalyzability, and freedom from a discernible red or green component so vehemently reiterated by Ladd-Franklin in chorus with certain of the older Hering adherents.[8] To unprejudiced perception, the same criterion of uniqueness or singleness may be applied to any one of the 150 or more hue strips of the spectrum or continuum—violet, orange, purple—when viewed in isolation, as Hering himself frankly acknowledged.

The claim of yellow to genetic primacy,[9] in the opinion of experienced laboratory workers, familiar with the functioning of both normal and deficient color systems, rests on other grounds. The preëmption of the field of vision of the normal daylight eye by yellow, photo-receptors for which are distributed far out toward the margin of the retina,[10] the wide span within the spectrum of stimulus frequencies adequate to evoke it (5000-8000 ångstroms and beyond); its dominance in the color world of the commoner types of chromatic deficiency—the so-called red-green blind who match the bulk of their color impressions with varying saturations and shades of yellow, *i.e.* a hue whose most effective stimulus is located in the strip of wave-lengths just below sodium yellow in the spectrum, 590 to 570 mμ; the stability of the yellow mechanism under intensive stimulation or disease, all support its claims.[11] It seems indeed highly probable that yellow may represent the oldest visual response, possibly antedating white; occupying originally, with its comple-

laston's results and their bearing. See R. A. Houstoun, *Vision and Color Vision*, 1932, 184.

[8] Cf. D. Katzin and E. Murray, Instability in color-naming in normal and anomalous observers, this JOURNAL, 46, 1934, 611.

[9] The popularization of this idea, borrowed from Donders, constitutes Ladd-Franklin's chief claim to rank among color theorists. Her elaboration of the genetic standpoint, in attempted reconciliation of Hering and Helmholtzian tenets, has never worked smoothly in detail as an explanatory principle. Black, color contrast and after-image, scoterythrous color-blindness, are among the phenomena left unaccounted for in her schematic hypotheses. Though accepted uncritically at face value by many psychologists, the whole structure falls to the ground with her shift of the red and green primaries (associated with the R and G molecules the evolution of which from the more primitive Y molecule she posits) from yellow-green and yellowish red to verdigris and crimson. The stimulus pair that yield the sickly mixture 'yellow' of the Helmholtzians are simply not the pair yoked in the complementary phenomena of color vision. See E. Murray's review of Ladd-Franklin's *Colour and Colour Theories*, this JOURNAL, 41, 1929, 653-656.

[10] It seems highly probable that the ambiguous responses to high intensity 'red' stimuli reported occasionally in the periphery, but apparently unguarded from a 'set' for red, are actually responses of these receptors, and liminal *yellow* rather than red in hue. Similarly the response to radiations between 800 and 856 mμ, in the so-called infra-red, appears to be yellowish rather than red in tone, even when stray light is excluded by filters.

[11] The occurrence of 'yellow' where primary 'red' is in abeyance or missing, as in the peripheral field or in red-green blindness, can be explained by the trichromatist only by complicated secondary mechanisms operative on the cerebral level. The different neural levels of the retina, along with slight structural variations in the cones, apparently suffice, however, for tetrachromatic theory—which, moreover, was never linked unalterably by its originator with *three Sehsubstanze*.

mentary blue, the entire span of the spectrum.[13] Certain groups of facts suggest that only as the eye developed protective pigments, or altered climatic conditions reduced light intensities did special red and green response-mechanisms (never very stably established) arise, cutting in on the curves for yellow and blue sensitivity, especially under moderate or reduced illumination.[13]

Luckily there are today many indications of a saner attitude toward color facts and color theory, a tendency to accept the eye as it is, rather than as it ought to be in the interests of a fancied 'economy.' If this permits the assembling of data unwarped by *a priori* mathematical formulas, it should bring both a better basis for the interpretation of color-blindness and its genetic correlates, and a better foundation for the integration of neural theory and the sense of vision.[14]

The recognition that every hue as experienced requires tri-dimensional definition, that it varies in depth of color or chroma as well as in intensity or brightness, signalized in Troland's Report of the Colorimetry Committee in 1922, could but have wide repercussions. Practically demonstrated in Munsell's widely accepted color system and color handbooks, it is now in use in the revision of standard color terms under way for the U. S. Pharmacopaeia at the hands of Inter-Society Color Council and Bureau of Standard experts. Not only has the confusion of saturation or chroma with intensity or 'luminosity' by physicists long muddled the use of the color triangle, and obscured the fact that dichromats ordinarily distinguish red and green as different *saturations* rather than brightnesses of yellow; it has also led to slurring over the important point that the mixture yellow of the Helmholtzians is far from rivalling in chromaticity the hue produced directly from wave-lengths 570-590. Recognition of this attribute should break down the stubborn insistence on the color-blend character of yellow,[15] and lead to the abandonment of the use of the Rayleigh equation and the Nagel anomaloscope as measures of the red or green sensitivity of an observer. A direct measure of the thresholds for red and green should give less ambiguous results. Variation in the yellow valence of either stimulus

[13] The fact that the most effective stimuli for yellow are located precisely within the strip of radiations of greatest photo-effectiveness for achromatic response in the daylight eye is not without significance here. The statement that the most effective achromatic wave length lies somewhere near 545 mμ is based apparently upon scotopic vision. The brightest strip for Hecht's two deuteranopes lay near 570 for the photopic eye.

[13] This difference in the intensity levels of the two sets of thresholds may have inspired certain recent attempts to frame a 'quantitative theory' of color; by theorizers, however, too little familiar with the complexities of color phenomena to coördinate fact and hypothesis.

[14] Realization by progressive physicists that their color-mixing curves and formulæ, however ideally simple and compact, are not adequate to the physiological properties of the eye; and further, that the terms red-blind and green-blind are utterly misleading, since a blue-yellow spectrum linked with red-green deficiency characterizes both the commoner types, has lodged many of them in a dilemma. Like the scientific-minded schoolmen of the Middle Ages, forced to reconcile the contradictory tenets of science and theology, they appear to have taken refuge in the doctrine of the *Twofold Truth; i.e.* what is acceptable in color-mixing formulae is not necessarily theoretically true. If, however, they decide to retain Young's somewhat casually set up tri-receptor doctrine, they must assume in color-blindness not a reversion to a simpler type of vision but Fick's complicated perversion of the red and green receptors, greatly to the damage of the vaunted simplicity and economy of the trichromatic theory.

[15] Cf. Houstoun's position on this point, *op. cit.*, 224 f.

component is sufficient to obscure variation in red or green responsiveness *per se.*

The significance of the outspoken abandonment of the three-color theory as mathematically unsound by so distinguished a physicist and authority on light as Houstoun should go far to turn speculation into more productive channels.[16] Economy of operation with three ordinates or stimuli, the lack of satisfactory dyes to produce a clear yellow glass or filter, the psychologist's usual poverty of physical resources and laboratory apparatus have long retarded the observations necessary to establish yellow as a primary. With the extended use of color in industry, however, has come the insistent demand for a more adequate basis for colorimetric technique and matching devices. Already the spectral curve for a given color sample turned out by the new spectrophotometers has been found to afford a more satisfactory specification for reproduction than the older tri-stimulus formulas. But interpretation of the significance of these curves for the eye demands an understanding of the latter, and its sensitivity curves for the various wave lengths. It demands a knowledge also of complementary colors and the fashion in which when reflected from a given surface they cancel out; of the conditions favorable to high saturations for each hue—for chroma is not a function of purity only as many physicists now assume—nor are the conditions favorable identical from hue to hue.

The *Eichung* of the spectrum in tetrachromatic terms is at present the urgent need, in both the theoretical and the practical fields. The requisite apparatus—two spectroscopes or spectrophotometers and accessories—is rarely available to a psychologist convinced of the need for matching each strip of the color circle with the proper mixture of the tetrachromatic primaries, Y, R, B, and G (the latter chosen near 505 mμ, as König, checking his mixture ordinates with dichromats, admitted as early as 1892). It is possible these curves might serve both as mixture or colorimetry curves, and also as indicators of the points of maximal sensitivity on the spectrum wave length abscissa for the four primaries. Such curves formed for twenty years the preoccupation of a Swedish scientist, Gösta Grönberg, but his untimely death in 1935 left us with only a meager and schematic account of his data and theory.[17]

As long as psychology delays equipping its personnel with the necessary exact knowledge of the intricacies of color phenomena and of the handling and control of light stimuli, and postpones attacking the problem of the quantitative testing and expression of the tetrachromatic theory, both accurate color observations in color-blindness and the development of adequate neural theories will be retarded. We shall be at the mercy of every distortion and elaboration of trichromatic theory (in defiance of the original argument of economy), every attempt such as the above to derive yellow from the operation of two distinct mechanisms.

Cornell University ELSIE MURRAY

[16] Houstoun, *op. cit.*, 223 ff. For review of the same see E. Murray, this JOURNAL, 46, 1934, 346-348.

[17] En ny fargteori, *Svenska Läkar. Föhandl.*, 20, 1923, 577-582. Grönberg posited four different types of cones, for R, G, Y and B, differentiated for the corresponding wave lengths of light by slight variations in their lamellar or spiral structure, the pitch of which had been found by Hesse to be of the order of magnitude of a wave length of light. Assuming secondary maxima, or resonance to wave lengths standing in simple ratios as 3 to 2 or 4 to 5, Grönberg claimed his theory explained not only the reëmergence of red in the violet, and lavender gray at the end of the spectrum, but also the unexpected brightness of certain mixtures, and the Bidwell and Benham rings.

On the Fallacy of Imputing the Correlations Found for Groups to the Individuals or Smaller Groups Composing Them

If the correlation between two traits, A and B (say, poverty and delinquency), in n groups (say, the residents of w districts) has a certain value, K, the correlation between A and B in the individuals or the families composing the groups need not be K and will not be, save in very special circumstances. It will usually be very much nearer zero. This is easily demonstrable, yet some able teachers have been guilty of thinking that the two correlations will be closely similar, or of leaving their students relatively unprotected against thinking so. And it is to be feared that many readers with little knowledge of correlation often misapply correlations between features of states, counties, cities, wards, classes, etc. to their constituent units.

Consider, for example, the following statements by Cyril Burt, an expert in psychology and education.[1] They are not downright guilty of the fallacy, but just what does he wish his readers to think? Can he justly complain if people quote him as asserting that certain social conditions correlate 0.60 or 0.70 with juvenile crime?

"Table V [not shown here] shows the percentages of juvenile delinquency in each of the twenty-nine Metropolitan boroughs, treating for these purposes the City of London as itself an additional borough. Side by side, for every area I have placed such accessible statistics as might be held to reflect light on its social character. These in the main relate to features I have already commented upon in passing—the birth rate, the death rate, the amount of overcrowding, the amount of open space, the proportion of illegitimate children, the proportion of persons in receipt of relief, the percentage of the child population scheduled for purposes of public elementary education, and finally, the figures for poverty calculated by Charles Booth.

"The correlations between the figures for juvenile delinquency and these several statistical assessments are . . . sufficient to show that the connection of childish offenses with poverty and its concomitants is significantly high, though not so high perhaps as previous inquirers, trusting mainly to experience and general impression, have as a rule implied. With the amount of poverty in each borough, as assessed by Charles Booth's survey, the amount of juvenile delinquency is correlated to the extent of 0.67, with the relative amount of poor relief to the extent of nearly 0.50; with the number of children scheduled for school purposes (probably the best single index of the present social character of each borough as a whole) to the extent of 0.63. The highest coefficient of all is that for the correlation between juvenile delinquency and overcrowding, namely 0.77. The correlation with the death rate (0.70) is nearly as high. Allowing for the gross shortcomings, inevitable in estimates so crude, so vague, and in some cases so largely out of date, these several figures are remarkably consistent one with another. They indicate plainly that it is in the poor, overcrowded, insanitary households, where families are huge, where the children are dependent solely on the State for their education and where the parents are largely dependent on charity and relief for their own maintenance, that juvenile delinquency is most rife."[2]

There is every reason to believe that the correlation between the overcrowding *of each family* and its proportion of delinquents per child would be much below 0.77; that the correlation between the poverty of each family and its proportion of delinquents per child would be much below 0.67; and similarly for the other correlations. Burt's statement elsewhere that 19% of his juvenile delinquents came from

[1] If the fallacy was encouraged only by incompetent scientists, a note about it in this JOURNAL would not be needed.

[2] Cyril Burt, *The Young Delinquent*, 1935, 72-75.

TABLES I–XII

THE CORRELATION (ZERO) BETWEEN INTELLIGENCE QUOTIENT AND NUMBER OF ROOMS PER PERSON FOR EACH OF TWELVE DISTRICTS

The captions at the top of the Tables, −4, −3, −2, −1, 0, 1, 2, 3, 4 and 5, represent intelligence quotients of 64 to 71, 72 to 79, 80 to 87, etc. The stubs at the left of the Tables, −4, −3, −2, −1, 0, 1, 2, 3, 4 and 5, represent 0.20 to 0.39 rooms per person, 0.40 to 0.59 rooms per person, 0.60 to 0.79 rooms per person, etc.

Table I

	−4	−3	−2	−1	0	1	2	3	4
−4									
−3				1	1	1			
−2		1	1	4	6	4	1	1	
−1	1	2	4	7	15	7	4	2	1
0	2	2	6	8	20	8	6	2	2
1	1	2	4	7	15	7	4	2	1
2		1	1	4	6	4	1	1	
3				1	1	1			

Table II

	−4	−3	−2	−1	0	1	2
−4							
−3			1	1	1	1	
−2		1	2	4	4	2	1
−1		1	3	6	6	3	1
0		1	2	4	4	2	1
1			1	1	1	1	
2							
3							

Table III

	−4	−3	−2	−1	0	1	2	3	4
−4									
−3									
−2				1	1	1	1		
−1		1	1	2	5	5	2	1	
0		1	3	5	10	10	5	3	2
1		1	3	5	10	10	5	3	2
2		1	1	2	5	5	2	1	
3				1	1	1	1		

Table IV

	−4	−3	−2	−1	0	1	2
−4							
−3		1	1	1	1		
−2	1	2	4	4	2	1	
−1	1	3	6	6	3	1	
0	1	2	4	4	2	1	
1		1	1	1	1		

Table V

	−4	−3	−2	−1	0	1	2	3	4
−4		1	1	1					
−3	1	2	4	2	1				
−2	1	3	6	3	1				
−1	1	2	4	2	1				
0		1	1	1					

Table VI

	−4	−3	−2	−1	0	1	2
−4			1	1	1		
−3		1	2	4	2	1	
−2		1	3	6	3	1	
−1		1	2	4	2	1	
0			1	1	1		

Table VII

	−4	−3	−2	−1	0	1	2
−4							
−3			1	1	1		
−2		1	2	4	2	1	
−1		1	3	6	3	1	
0		1	2	4	2	1	
1			1	1	1		

Table VIII

	−3	−2	−1	0	1	2	3
−4			1	1	1		
−3		1	2	4	2	1	
−2		1	3	6	3	1	
−1		1	2	4	2	1	
0			1	1	1		

Table IX

	−4	−3	−2	−1	0	1	2
−4							
−3							
−2				1	1	1	
−1			1	2	4	2	1
−0			1	3	6	3	1
1			1	2	4	2	1
2				1	1	1	

Table X

	−3	−2	−1	0	1	2	3
−4							
−3							
−2							
−1				1	1	1	
0			1	2	4	2	1
1			1	3	6	3	1
2			1	2	4	2	1
3				1	1	1	

Table XI

	−4	−3	−2	−1	0	1	2	3	4
−1									
0						1	1	1	
1					1	2	4	2	1
2					1	3	6	3	1
3					1	2	4	2	1
4						1	1	1	
5									

Table XII

	−3	−2	−1	0	1	2	3	4	5
−1									
0									
1						1	1	1	
2					1	2	4	2	1
3					1	3	6	3	1
4					1	2	4	2	1
5						1	1	1	

homes of extreme poverty (classes A and B) and 53% from classes A, B, C and D, whereas only 8% of the general population are from classes A and B and only 30% from classes A, B, C and D, would give a correlation between delinquency and poverty not of 0.67, but of about 0.18.

I append an artificial illustration of the general fact, which will be of service especially in making the matter clear to students. A is supposed to be intelligence quotient and B is supposed to be the fraction of a room or number of rooms per person.

Table XIII

COMBINATION OF ALL THE PERSONS IN TABLES I-XII

	−4	−3	−2	−1	0	1	2	3	4	5
−4		1	2	3	2	1				
−3	1	4	10	12	11	5	1			
−2	2	8	19	30	26	14	4	1		
−1	3	11	25	38	44	23	9	3	1	
0	3	7	21	33	50	29	15	6	4	
1	1	3	12	20	39	26	16	8	5	
2		2	3	9	17	16	12	9	3	1
3				3	4	6	8	8	4	1
4						2	3	5	2	1
5							1	1	1	

Table XIV

AVERAGES OF THE TWELVE DISTRICTS

	−3	−2½	−2	−1½	−1	−½	0	½	1	1½	2	2½	3
−3													
−2½													
−2			1			1		1					
−1½													
−1					1	1	1						
−½													
0							2						
½								1					
1									1				
1½													
2											1		
2½													
3													1

Let the scores for sample persons in each of twelve districts into which a city is divided be as shown in Tables I-XII. Within each of the districts the correlation between A and B is zero. If all the persons in the sample are combined, the result is Table XIII, and the correlation is 0.45. If the averages for the twelve districts are used, the result is Table XIV, and the correlation is 0.90.

Columbia University E. L. THORNDIKE

The Stepwise Phenomenon

If an animal be trained to approach a gray of medium brightness (*M*) in preference to one of less brightness (*L*), it will tend, upon the substitution for *L* of a gray (*H*) of a brightness still greater than *M*, to approach *H* more frequently than *M*. This has been designated the 'stepwise phenomenon' by members of the Gestalt school, and it has been explained as the animal's response to 'relative' rather than to 'absolute' brightness of the grays (cf. Koffka, and Köhler).[1] Although the experimental results have in the main confirmed the facts outlined, four aspects require further consideration.

In the first place, in the substitution-series not all the test-runs are approaches to gray *H*. The writer has shown elsewhere that one may expect a range of relative approaches which extends from less than 50% to 95%.[2] The results reported by other investigators seem to confirm these figures.[3] May it be said that the animals responding 'relatively' 95% of the time are any more typical than those so responding only half the time? If not, any adequate explanation should fit equally well for both cases.

In the second place, there are now at least two negative bits of evidence to consider. Taylor has shown that when *L* is black, and *H* white, with *M* a medium gray, 'asbolute' and not 'relative' approaches are made by chickens in the substitution series.[4] Recently Ohtsuka has demonstrated that for monkeys a relatively great separation of the brightnesses of the grays will lead to 'absolute' approaches, whereas grays closer together in brightness will lead to 'relative' approaches.[5] Thus the relative intensities of the stimuli are of importance in determining what the animal will do in the substitution series.

The third aspect concerns the effects of 'overtraining' or experience in the situation after *S* has reached the criterion of learning. The writer failed to find any significant shift in the performance of rats when overtrained 100 trials beyond 'learning.'[6] Jackson and his colleagues,[7] using children as *Ss* and different sizes of wooden forms, rather than grays, as the variables, have, however, found such a shift. In their experiment, overtraining resulted in an increase in the number of 'absolute' choices. Although the groups were small and later results may thus fail to confirm these findings, it seems not unlikely that the amount of experience in the training situation

[1] K. Koffka, Perception: An introduction to *Gestalt-Theorie, Psychol. Bull.*, 19, 1922, 531-585; W. Köhler, Nachweis einfacher Struktur-funktionen beim Schimpansen und beim Haushuhn, *Abh. d. königl. Preuss. Akad. d. Wissensch., Phys.-Math. Klasse*, 1918, Nr. 2.

[2] The 'stepwise phenomenon' in rats, *J. Comp. Psychol.* (in press).

[3] A. H. Gayton, the discrimination of relative and absolute stimuli by albino rats, *ibid.*, 7, 1927, 93-105; C. J. Warden and B. J. Rowley, The Discrimination of absolute versus relative brightness in the ring dove, Turtur risorius, *ibid.*, 9, 1929, 317-337; D. O. Hebb, The innate organization of visual activity: II. Transfer of response in the discrimination of brightness and size by rats reared in total darkness, *ibid.*, 24, 1937, 277-299.

[4] H. Taylor, A study of configuration learning, *ibid.*, 13, 1932, 19-26.

[5] N. Ohtsuka, Über die absolute und relative Wahl beim Affen *Cercopithecus sp.*, *Acta. psychol. Keijo*, 3, 1937, 33-44 (Abstract seen).

[6] *Op. cit.*

[7] T. A. Jackson, E. Stonex, E. Lane, and K. Dominguez, Studies in the transposition of learning by children: I. Relative vs. absolute response as a function of amount of training, *J. Exper. Psychol.*, 23, 1938, 578-600.

has an effect upon what the organism does subsequently in the substitution series.

In the last place, Jackson and his associates have also shown that the reaction-time of *S* is greater in the substitution than in the training trials.[8] It is to be regretted that more experimenters have not obtained measures of the reaction-times of their *Ss*, since such data are indicative of whether or not the two situations are perceptually identical. That is, if *S* does not perceive any change in the situation, we would expect his reaction-time to remain constant. On the basis of the results cited here, it would seem that the training and transfer stimuli are, at least under these conditions, perceived as different.

It would seem obvious that any explanation of the performance of the animals or children merely on the basis of an essentially identical perceptual structure in the training and substitution-situations is impossible. The Gestalt explanation fails to account for the wide individual differences among the *Ss*, the dependence of performance upon the relative differences among the stimuli, the effect of overtraining, or the increased reaction-times in the substitution-series. Jackson and his collaborators have proposed an explanation based upon the concept of 'generalization' of the 'correctness' or 'incorrectness' of a given stimulus.[9] Although this explanation is appealing because of its statement in mathematical form it is difficult to conceive of any process going on within an organism which would correspond to the theoretical curves.

In the consideration of a possible explanation for the behavior as outlined, the arguments of Gundlach and Herrington deserve attention.[10] Discrimination of the relative brightness of two grays is of course easier when the stimuli are presented simultaneously (cf. Bentley).[11] That is, the just noticeable difference for brightness attains its minimum value in the case of simultaneous stimulation, where a direct comparison is possible. If an *S* be required to discriminate between grays which are presented, not simultaneously, but a day or so apart, his difference limen will of necessity rise. The latter situation is, in effect, the one presented to the animals in the 'transfer' or *M-H* situation. Especially when we remember that we have no way of instructing the animals to look out for changes in the brightnesses of the grays it is not surprising that the change, when small, is overlooked. Indeed, we might well expect less than absolute accuracy even in the presence of suitable instruction, in the case of grays close together in brightness. The above discussion will apply equally as well to the experiments using children as *Ss*, since instructions of the sort mentioned must be avoided for fear of biasing the results. The discussion is also germane in the case of stimuli differing in some property other than brightness, as, for example, size. We may logically expect, then, that the greater the differences among *L*, *M*, and *H*, the more 'absolute' approaches will be made.

Since formal instruction is impossible in the animal experiment, the situation should be arranged so as to include the items to be 'learned' and the factors necessary to insure that the animal responds in terms of the problem set by *E*. An animal 'learning' experiment, then, is not strictly analogous to the human association of

[8] *Op. cit.*
[9] *Op. cit.*
[10] R. H. Gundlach and G. B. Herrington, Jr., The problem of relative and absolute transfer of discrimination, *J. Comp. Psychol.*, 16, 1933, 199-206.
[11] M. Bentley, The memory image and its qualitative fidelity, this JOURNAL, 11, 1899, 1-48.

nonsense syllables, or other forms of rote learning. The consequence of this consideration is that the animal is performing in *its* psychological environment, not in that of the experimenter. Hence at least the early stages of the animal's experience in the situation must be given over to the discovery of just what variables in the environment will serve as reliable indicators or cues of things to come. Among the various ways of making it probable that the animal is responding in terms of those portions of the situation which *E* considers critical are 'tuning' (as by deprivations of food) and strict limitation of the possible cues. In the type of situation under consideration (*i.e.* differential response to brightnesses), the animal is 'tuned' to approach food, and the only reliable cues to the position of the food are the doors or papers of varying brightness. The animal ultimately comes to approach one brightness instead of the other. The cues utilized by the animal may be of two kinds. First, the 'correct' brightness may be selected by a comparison of one brightness with the other. Secondly, a given brightness may be perceived directly as 'correct' or 'incorrect' by the relation of the given brightness to its background. It may be that when the grays are relatively close together in brightness the animal is forced to the comparison, due to the difficulty of discrimination for stimuli not simultaneously present, as indicated earlier in this note. The situation is analogous to that involved in relative as against absolute judgment of pitch. If in this latter case, the *M-H* situation replaces the *L-M*, the change will be overlooked, as previously suggested, and what appears to be a 'relative' approach will occur. That is, the performance of a given animal in the transfer series will depend upon the manner in which it has come to perceive the situation. With any given set of brightnesses, individual differences will arise due to (1) the threshold of the given animal, and (2) the 'set' or 'hypothesis' which the animal brings to the situation (cf. Krechevsky).[12] In the case of the higher mammals 'set' is of extreme importance, and it is probably a shift in this factor which is at the basis of the results obtained by Jackson.

Throughout this note the discussion has been confined largely to the case of brightness discrimination. This is for two reasons. First, nearly all the experimental work has been done with varying degrees of brightness as the variables. Secondly, the discussion can readily be extended to the cases involving similar gradations in any attribute of the situation. It is unfortunate that the experimental work has been so sharply limited in scope. There remains the problem of its extension into other sense fields. Also, owing apparently to a disregard of negative cases, there remains the problem of determining under just what experimental conditions the 'relative' or the 'absolute' response will predominate. Many more experiments, shrewdly planned, are necessary before we understand the problem of perceiving in animals.

Cornell University F. NOWELL JONES

THE FORTY-SIXTH ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

The Forty-Sixth Annual Meeting of the American Psychological Association was held at Ohio State University, Columbus, Ohio, on Wednesday, Thursday, Friday, and Saturday, September 7, 8, 9, and 10, 1938. A total of 1153 persons registered,

[12] I. Krechevsky, 'Hypothesis' vs. 'chance' in the pre-solution period in sensory discrimination-learning, *Univ. Calif. Publ. Psychol.*, 6, 1932, 27-44.

classified as follows: 239 Members, 495 Associate Members, 66 newly-elected Associates, and 353 persons not affiliated with the Association.

At the Business Meeting one Member was elected, 18 Associates transferred to the status of Member, and 250 Associate Members were elected.

Newly-elected officers are: President, Gordon W. Allport; Directors, Frank N. Freeman and Henry E. Garrett; Division of Anthropology and Psychology of the National Research Council, Harold E. Burtt, Florence E. Goodenough, and Walter R. Miles; Social Science Research Council, Richard M. Elliott; American Association for the Advancement of Science, Walter R. Miles and John A. McGeoch.

Karl F. Muenzinger was appointed as a new member of the Committee on Precautions in Animal Experimentation. Henry E. Garrett and Walter R. Miles were continued as representatives on the Joint Committee with the Psychologists League. John E. Anderson was continued as representative to the American Documentation Institute. John G. Jenkins was named representative to the New York Management Council.

Calvin P. Stone, of Stanford University, and Warner Brown, of the University of California, were appointed as members of the Executive Committee for the year 1938-1939 in anticipation of the Forty-Seventh Annual Meeting to be held in California sometime during the week beginning September 3, 1939. The Program Committee for the 1939 meeting will consist of Elmer K. Culler, Chairman, Edward B. Greene, and Willard C. Olson, Secretary.

An important decision affecting the members was made when it was voted, upon recommendation of the Council of Directors, to send the Psychological Bulletin to all Members and Associates beginning with the issue of January, 1939. Under this plan future programs will be printed as the July issue of the Bulletin, the proceedings will appear in November as usual, and the presidential address will be placed in the following January issue. Economies introduced permitted this action without a change in dues.

The Council of Directors announced the completion of the dissolution of the Psychological Review Company under the date of August 13, 1938. All publication matters will hereafter be conducted in the name of the Association.

The American Association for Applied Psychology, the Midwestern Psychological Association, and the Western Psychological Association were affiliated with the American Psychological Association in accordance with the provisions of Article XI of the By-laws.

The Association voted to rename the "Honorary Member" classification "Life Member" because of the automatic nature of the requirements set up in the By-Laws at the 1937 meeting.

The Association created a new Committee on Scientific and Professional Ethics to consider the advisability of drafting an ethical code the purpose of which would be to serve as a guide to Members and Associates. Robert S. Woodworth was made chairman of the committee, with additional members to be appointed. The Association also created a Committee on Press Relations, with B. F. Skinner as chairman and Donald G. Paterson and Horace B. English as members. The Council created a Committee on Assistance in Employment of Refugee Psychologists, with Gordon W. Allport as chairman and members to be appointed.

The Association created a Committee on Investments, with Willard L. Valentine

as chairman and Leonard Carmichael and Samuel W. Fernberger as members. An Advisory Committee to work with A. T. Poffenberger and his associates in the construction of an alphabetical cumulative index for the Psychological Index was appointed, with A. T. Poffenberger as chairman and Karl M. Dallenbach, C. M. Louttit, and R. R. Willoughby as members.

A survey on "The Teaching of Psychology in Teachers Colleges" was presented by Dr. Arthur I. Gates for the Committee to Study Problems Connected with the Teaching of Psychology in High Schools and Junior Colleges, of which Goodwin B. Watson was chairman and Calvin P. Stone an additional member. The survey of the teachers colleges was added to the committee's assignment at the 1937 meeting. The Association voted to have one thousand copies of the report printed and distributed to presidents and teachers of psychology in teachers colleges.

The Association voted to re-appoint its delegates to the Inter-Society Color Council and to act as sponsor, without financial obligation, to the 1939 meeting of the Inter-Society Color Council.

The Council of Directors had received copies of resolutions protesting the holding of the next International Congress of Psychology at Vienna from various individuals and organizations including the Chicago Psychological Club, the Illinois Society of Consulting Psychologists, the Midwestern Psychological Association, the Psychologists' League, the Rocky Mountain Branch of the American Psychological Association, and the Western Psychological Association. On the recommendation of the Council of Directors the Association voted to adopt the following resolution:

Be it resolved, That the American Psychological Association request the Committee in charge of arrangements for the Twelfth International Congress of Psychology to terminate, if it has not already done so, the tentative plan to hold this congress in Vienna in 1940; and that it request the Committee to arrange for the Congress in some country where the progress of psychology as a branch of science is not hindered by a government hostile to the tradition of free and unimpeded scholarship.

The Psychometric Society arranged a scientific session on "Psychometrics" and a Round Table on "Factor Analysis" on Thursday in collaboration with the Program Committee of the American Psychological Association. Two hundred and one persons registered for the meetings. The Business Meeting of the Society was held on Wednesday. Officers continued for the coming year are: President, Joy Paul Guilford; Secretary, Jack W. Dunlap; and Treasurer, Albert K. Kurtz.

The National Institute of Psychology held sessions at 4:00 on Wednesday and Thursday. The Business Meeting was held at 5:00 P.M. on Wednesday. The officers for 1936-1941 are: President, Samuel W. Fernberger; Vice-President, F. L. Wells; Secretary-Treasurer, Karl M. Dallenbach; Directors, Franklin Fearing and Herbert Woodrow.

The Society for the Psychological Study of Social Issues arranged a scientific session on the "Psychology of Social Institutions" for Thursday in collaboration with the Program Committee of the American Psychological Association. Two hundred and fifteen persons registered for the meetings. At the Annual Business Meeting on Tuesday George W. Hartmann of Columbia University was elected Chairman for 1938-39 with G. W. Allport, F. H. Allport, Gardner Murphy, and E. C. Tolman as new members of the Council for the term 1938-40. I. Krechevsky of the University of Colorado was continued as Secretary-Treasurer. The same evening Gardner

Murphy delivered the presidential address on the subject "The Research Task of Social Psychology."

The American Association for Applied Psychology met September 5, 6, 7, and 8, 1938, in conjunction with the Association. Five hundred and thirty persons registered for the meetings. Besides the four research programs (listing 32 papers) arranged in collaboration with the Program Committee of the American Psychological Association, there were seven symposia and a round table, a public meeting with five addresses, a recessed organization meeting and the regular business meeting, and a large and varied array of conferences dealing with professional problems. Following the annual dinner the retiring President, Douglas Fryer, delivered an address on the subject "Applied and Professional Attitudes." Professor Donald G. Paterson was elected President, Dr. Horace B. English, Executive Secretary, and Dr. Jack W. Dunlap, Treasurer.

The program of the American Psychological Association consisted of twenty-one scientific sessions in which 172 papers were presented by Members and Associates. Six round tables were arranged for Thursday and Friday and fourteen research and instructional films were scheduled for Wednesday evening.

The Presidential Address was delivered by John Frederick Dashiell of the University of North Carolina at the general session in the Chapel of University Hall on Friday on the subject "Some Rapprochements in Contemporary Psychology." Members of the Department of Psychology entertained members and guests at the Faculty Club at the conclusion of the general session.

Excellent facilities for the meeting were provided by the University. The Faculty Club, Dormitories, Gymnasium, Swimming Pools, Tennis Courts, and Golf Course were opened to the Association. Professor Harold E. Burtt as Chairman, his colleagues, and many administrative officials earned the well-deserved thanks of the Association.

University of Michigan WILLARD C. OLSON

THE 1939 MEETING OF THE INTER-SOCIETY COLOR COUNCIL

The annual meeting of the Inter-Society Color Council will be held in New York, Feb. 23, in the auditorium of the Electrical and Gas Association in Grand Central Palace. The program of the afternoon session is sponsored by the American Psychological Association and will deal with the psychological, physical and practical aspects of the problem of "Color Tolerances." In the evening, each one of the member societies will present a short popular demonstration of some aspect of its work with color.

Members of affiliated societies are welcome to attend the programs but, since seating facilities are limited, they should secure admission cards in advance from the chairman of their delegation.

Hobart College FORREST LEE DIMMICK

BOOK REVIEWS

Experimental Psychology. By ROBERT S. WOODWORTH. New York, Henry Holt and Co., 1938. Pp. xi, 889.

With the publication of this book, Woodworth becomes the chief encyclopedist of American psychology, for there has not been, in psychology's central sector, a book of such wide scope, implemented by such multitudinous detail, since Wundt's *Physiologische Psychologie,* nor, I should think, a book of such meticulous wisdom since Titchener's *Manuals.* Covering as it does, without systematic bias, almost the whole range of what is loosely called *general psychology,* this volume should have a marked effect upon graduate instruction in psychology. It is, in short, *the* definitive book in general psychology, and its publication, so long awaited, is an event of the first importance.

The text could have been divided into four main parts as follows: I: Learning and memory (memory, retention, memory for form, conditioned response, maze learning, practice, skill, economy, interference): 229 pp. or 28%. II: Reaction and feeling (simple feeling, facial expression, bodily changes in emotion, psychogalvanic reflex, reaction-time, associative reaction, esthetic preference): 158 pp. or 19%. III: Sensation and perception (psychophysical methods, psychophysical data, skin senses, smell, taste, hearing, sight, eye-movements, color, perception of form, visual space perception, apprehension): 321 pp. or 39%. IV: Thought (reading, problem solving, thinking): 111 pp. or 14%.

Woodworth defines experimental psychology as the empirical field where there is control of the independent variable. On this ground he excludes from his book all the research where the 'correlational method' is used, an exclusion that makes his undertaking more nearly possible. He also omits, only partly for the same reason, the data of child development and the data of psychotherapy. Doubtless he will be accused of bigotry in applying the title *experimental psychology* to so limited a field, but he has a sanction of usage in spite of the persistent protests from psychology's less aged periphery.

It is a large book—823 pages of text—about 425,000 words.

Inevitably one asks whether such a book is up-to-date. Woodworth himself, in his preface (p. iii), examines the dates of the 1729 references that he gives. A third of them are for 1930 or later. A fourth of them are before 1900. The frequency function of citations is continuous: the older the fewer. These statistics, however, do not present the whole truth. Woodworth belongs to the school of historical thought that regards mutations of thought as abnormal. He readily sees the present in the past, and again and again he cites the recent studies as confirming or explicating some finding of 30 or 50 years ago. Of course antiquity varies with subject-matter. In the chapter on reaction-time, 82% of the references are dated before 1930, whereas in the chapter on the conditioned reflex, 83% are dated in 1930 or later. There are zealots who will find the chapters on learning and memory antiquated because they do not orient themselves with respect to the 'dynamic' concepts of motivation, but they will be unfair to Woodworth. He has set himself definitely to avoid speculative hypotheses and concepts that do not find ready meaning in terms of experimental operations. His chapter on the conditioned reflex, for instance, is really ultra-modern,

for it is almost a special chapter on motivation, although Woodworth does not mention that word. He is sticking to 'facts.'

On the other hand, by 'sticking to facts' Woodworth does not fail to achieve scientific generalizations. He is less opposed to hypothesis than to pedantry. In the art of the neutral approach, bare of the pedantic substantialized hocus-pocus with which psychological thought is habitually encumbered, he is past master. The way in which he persuades the reader that the conditioned reflex depends on the wishes of the subject and not on the frequency of the concurrence of stimuli (pp. 92-123), and that the rate of reading depends not on the speed of the eyes but on the speed of comprehension (pp. 722-744), exhibits this method in perfection.

Neutral approach. Woodworth's use of the 'neutral approach' to achieve scientific generalization while avoiding controversy and pedantry is so unusual as to need description. Here are six instances in which I think Woodworth makes a positive contribution in avoiding the witchery of word-magic.

(1) The chapter about the transfer of training (pp. 175-207) is a straightforward account of all the ways in which prior learning of one material affects subsequent learning of another material. By keeping to such an 'operational' definition of transfer, Woodworth avoids both the question of what is transferred and the problem of identical elements.

(2) Similarly the chapter on PGR (pp. 276-297) is simply a chapter about PGR, the way in which differences of potential can be maintained at different points on the skin—another operational definition. Woodworth himself thinks of PGR as meaning *psychogalvanic reflex,* but, if someone else wants to think of it as "perspiratory galvanic reflex," Woodworth not only sees no objection but presents the evidence for its being perspiratory.

(3) In discussing reaction-times Woodworth distinguishes between the simple reaction, where the stimulus and response are always the same, and the disjunctive reaction, where the stimuli ('discrimination') or the responses ('choice') vary. Disjunctive reactions take longer, and Woodworth is free to discuss the problem as to why they take longer because he avoids all the implications of the 'subtractive procedure' (pp. 333-339).

(4) The section on PSE ('point of subjective equality') as it varies in time (pp. 438-449) is splendid because it is simply about PSE, which is exactly what varies in time. All the speculative problems disappear. Is the variation a 'time-error'? Is it the variation of a 'trace'? Is the 'trace' physiological? If so, what is it physiologically? Or is the psychologist in making such observations really a physiologist? That is the kind of word-magic in which Woodworth does not deal.

(5) Another example of Woodworth at his best is his chapter on attention (pp. 684-712). Here he reduces attention to the capacity for simultaneous quick apprehension, which is what most of the experiments measure. With that conception he rids himself of everything connected with the question as to whether attention is a dynamic force or an observable sensory attribute.

(6) In his chapter on solving problems (pp. 746-782) he enters into the question of theory, as he did not in these other instances. He asks: is a problem solved by trial-and-error or by insight? In such a situation Woodworth is not an eclectic; he does not choose the 'better' view. Instead he discusses both views, introduces the experimental evidence, and ends by convincing the reader—at last he convinced me—

that the supposed dilemma was merely fancied, that there is no difference between the views. All solving of problems, he might have said, is by insight-and-error.

This then is the 'neutral approach' which characterizes the book. Woodworth does not, however, always live up to his own genius in this regard. His chapter on thinking (pp. 783-800) is, I think, a notable failure on his part to rid himself of the limitations of classical pedantry, for he gets himself involved with the problem of consciousness (cf. also pp. 818-823), a concept for which he has ready no simple clarifying definition like his equation of the memory trace to PSE. Without such a definition one may not say whether thought that lacks adequate imagery is conscious or unconscious. When Woodworth pleads, as he seems to me to be doing, for some kind of substantialized non-sensory conscious content, I do not know what he has in mind (cf. also p. 43 f.). If he had seriously developed his own response theory of consciousness, he might have made this chapter, like the others, superior to controversy about fundamentals.

There are, of course, many psychologists who will reverse my praise and blame in this matter of the 'neutral approach.' They will say that, without speculative hypothesis, science loses its breath of life. That may be true of science as a whole; nevertheless we need also an occasional assessment of the accumulation of fact in psychology, and that, I take it, is what Woodworth is trying to do. Even those who can not stand facts unless they are hung on graceful hypotheses can applaud Woodworth for doing well what he sets out to do; and, if they do not like 'neutrality,' now at last they have a book in which to sink their teeth.

Essay style. For all his 'neutral approach' Woodworth has not written a handbook of experimental psychology. Besides being too small for a handbook, this text repeatedly adopts the essay style. Thus in places the exposition becomes unbalanced, occasionally overweighted. Woodworth, himself intrigued by the matter in hand, lets himself go, until the result, though often an excellent essay, is too long or discursive for the company that it keeps. Let me give examples.

The discussion of the Gestalt theory and G. E. Müller's theory for the memory of visual form (pp. 80-84) seems to me unnecessary in a book that in general eschews theories. Unduly long in view of the fact that it achieves no positive conclusion is the essay on the nature of simple feeling (pp. 235-241). Likewise the essay on facial expression (pp. 242-254) is too discursive for this book. The reader infers from the opening paragraphs that the Boring-Titchener-Piderit model shows what it was meant to show and then has to be undeceived—a method that involves the bad pedagogy of associative inhibition. Henning's smell prism does not merit 8 pages of discussion (pp. 483-490), nor should the essay on the localization of sound (pp. 518-534) receive 16 of the 38 pages allotted to the topic of hearing. The 4 pages devoted to the task of showing that the eye is not blind during eye-movement (pp. 591-594) are only 4 pages, but half a page would have done as well, except that Woodworth knows some "younger psychologists" who need convincing. The essay on visual space perception goes back to Leonardo and takes 12 pages to get past Panum (pp. 651-662). That is an historical luxury, although his concern with the remote past has some justification in the fact that he is devoting his chapter to showing that the visual perception of depth is purely visual and independent of accommodation and convergence. The essay on trial-and-error vs. insight (pp. 746-782), though pleasant enough reading, wastes precious space: the

conclusion is clear long before it is reached. Nor does the psychology of deduction (pp. 807-817) merit 11 pages. Counsel of perfection *ex post facto* is easy. No author, I suppose, ever neglects his favorite topics. The point here, however, is that the very style of this text—a text which is not a handbook—enables its author to indulge himself with such ease.

Since the text is not a handbook, would it not be better if it never tried to function as a handbook? The details of the computative procedures in the psychophysical methods (pp. 396-421) seem to me to be out of place. They and the Müller-Urban table are there to be used, but not to be read. If Woodworth felt that Spearman's methods have been neglected and should at last be given their due, if he felt that a table of the normal ogive with the SD as the argument ought to be more readily available, then he could have put all this material into an appendix, along with the mathematics of the experimental methods of esthetics (pp. 370-381).

Physiology. One of the most remarkable features of this book is its unphysiological nature. It is my belief that the words *neuron, synapse,* and *thalamus* do not occur in the book, nor the names of any of the endocrine glands. The cortex gets mentioned casually in connection with theories of vision. The chapter on hearing finds no need to mention the basilar membrane, and little to say of the organ of Corti. On the other hand, the rods and cones of the retina get their due, and fully as much is said about cutaneous receptors as present knowledge could justify. Some of these elementary facts are presumably presupposed. Certainly Woodworth takes it for granted that the all-or-none nature of nerve-conduction is known in advance to his readers, that and the facts of refractory period. On the function of the autonomic nervous system and the endocrine glands in emotion, he specifically refers to texts of Cannon, Bard and Dunbar (p. 257), limiting his own discussion of bodily changes in emotion to respiration and circulation. Perhaps the most explicitly physiological section in the book is the two pages on the neurophysiology of the PGR (pp. 282-284).

Has Woodworth omitted something essential? Yes and No. No psychologist nowadays would dare to know as little physiology as Woodworth seems to recommend. On the other hand, the concern of recent psychology for physiological detail has tended to take attention away from the mass of psychological fact that no physiologist ever considers. Woodworth's text, too unphysiological perhaps for a psychologist's Bible, nevertheless shows how very far the physiologists are as yet from being able to substitute for the psychologists.

Omissions. Besides this premeditated omission of physiological fact, there are a number of other gaps which can hardly be oversights. Pressure from the publishers would, for instance, have squeezed out whole topics more readily than it would have condensed the fluency of some of the essays. I list here six serious omissions.

(1) The text is inadequate in its discussion of somesthesis, largely because it limits itself to cutaneous sensibility. There is nothing about subcutaneous sensibility, kinesthesis, proprioception, hunger, thirst, and the other organic sensibilities. There is nothing about the 'static' sense except sixteen lines in the middle of the chapter on eye-movements. Nor is the discussion of cutaneous sensibility complete. I find nothing about after-sensations or about the perceptual fusions (wetness, roughness, stickiness, etc.). Paradoxical cold is mentioned casually and the Alrutz-Von Frey theory of heat not at all. While the neglect of the hypotheses of epicritic and protopathic sensibility pleases me, I nevertheless believe that the facts of returning sensibility after nerve injury are illuminating, Woodworth ignores them. I should like him also to say

something about Von Frey's discovery that the pressure thresholds for small stimuli are constant only if expressed in grm./mm. and not grm./mm.^{2}, and then he ought not to give these thresholds in grm./mm.^{2} as he does, citing Von Frey's early data (p. 455).

(2) There ought to be more than three pages (pp. 680-683) on the perception of motion. The subject has an instructive history and a considerable body of accepted fact. Being a topic that the physiologists neglect, it could well have been emphasized.

(3) There ought to be something—ten or fifteen pages at least—about the perception of time. There is nothing. The *Zeitsinn* used to be important in experimental psychology.

(4) The discussion of psychophysical methods and results omits all mention of the problem of equivalents and of equal sense-distances. This is a serious omission for it allows Woodworth to assume the approximate validity of Fechner's law, whereas, if he had had to give data on the variation of the j.n.d. in subjective size, he would not have left the matter in such a simple state. On the other hand, it may be said that some of these data are new, and no book can be quite up to the minute. (See Stevens and Davis, *Hearing*, 1938, 76-84, 94-97, 112-123, and references there cited from 1933 to 1937.)

(5) Except two sentences tucked away in a paragraph about color blindness (p. 556), there is nothing about chromatic sensibility in indirect vision.

(6) Even admitting that physiological fact is left out on principle, an exception might have been made in the case of the structure and functioning of the inner ear. So many facts of hearing begin to make sense when the nature of the sense-organ is known.

Specific criticisms. We come now to the specific criticisms. I want first to argue two important ones at length, and then briefly to list the rest, leaving the reader to form his own opinion of their importance.

(1) First of all let me deplore the fact that Woodworth teaches our students that Weber's law holds approximately in the middle ranges of intensity (pp. 430-438). I had hoped that Holway and Pratt (whom Woodworth cites in his bibliography) had definitely settled that notion. (Cf. also the Boring-Langfeld-Weld textbook, 196-202.) In every known case the Weber fraction decreases at first steadily as intensity increases. The function may level out as for tone, or it may 'kick up' again at the high intensities, as for brightness. The typical form is a curve with a determinable minimum. It shows thus best in a semi-log plot where the fraction is plotted against log *I*. Woodworth says that the use of a log scale "assumes Weber's law as a basis for arguing against it" (p. 433), but that is not true. The constant-increment arithmetic scale is the simplest frame of reference. The constant-ratio geometric scale is the next simplest, and the one to which we turn for exhibiting trends that will not show on the other. One would use semi-log coördinates to plot size of cities against their frequency, and assume nothing about Weber's law. Moreover, if the Weber function were plotted against arithmetic scales so that the curve at the low intensities was practically crowded out of the picture, then the 'kick up' at the high intensities (cf., *e.g.* König's data) would be exaggerated. Or, if one plotted only the middle portion on arithmetic scales, then one would naturally increase the scale for the Weber fraction, so that what had seemed to Woodworth approximately constant, would now be revealed as significantly curved, for the semi-log plot straightens out the middle as well as curving up the ends.

It is the acceptance of this faulty approximation that enables Woodworth to say that decibels measure equal sensation distances (pp. 325, 437). Actually curves have been drawn showing loudness scales in directly judged sense-distances, summed j.n.d.'s, and decibels, and the relation between no pair of these three measures is linear. (See Stevens and Davis, 118, 138 f.). Woodworth also says that Miss Gamble found that "Weber's law holds" for smell (p. 479). That is what Miss Gamble thought too, because the Weber fraction varied between 1/3 and 1/4 in 62% of the cases, and between 1/2 and 1/6 in 90% of the cases. We should hardly call that constancy nowadays.

(2) Although my next criticism appears to be only verbal, it is directed toward the confusion of thought which a wrong word yields. Woodworth, in discussing perception, uses the word *cue,* when he should say, I think, *cause.* Here are the instances. The localization of sound depends, he says, upon the binaural cues present in dichotic stimulation (pp. 525, 532 f.). He explains the tendency toward constancy of hue and brightness in colors seen under different illuminations as dependent on cues that the observer gets for his interpretation of the objective situation (p. 595). So too he accounts for the visual perception of depth as entirely a matter of the available cues that give information about the third dimension (pp. 651-680). One perceives words, he says, in accordance with the cues that the print provides (pp. 737-745). Woodworth thus gives the student a picture of a perceiving observer who gets all the cues he can from the total situation and reconstructs (or interprets, or infers to) objective reality from the data. Almost identically Helmholtz gave us for the explanation of perception the *unbewusster Schluss,* a concept that was generally abandoned sixty years ago, for the reason that there is no discernible interpretative process. It is true that Woodworth says: "We are not to suppose that the subject *notices* time or intensity differences and *infers* from them the location of sound. The small fractions of a ms. which are found to be effective direction cues are imperceptible as time-intervals. In order to approach any theory of sound localization, we must think in physiological terms" (p. 533). But similarly Helmholtz said that the *Schluss* was unconscious, instantaneous and compulsory—a contradictory statement.

Take what Woodworth calls the "conflict of cues." He says that, if an experimenter "applies equal intensity to the two ears along with a time difference, the intensity cue indicates a source in the median plane, while the time difference locates it on the side of the earlier reception of the sound. Which of the conflicting cues will the listener follow in his localization?" (p. 525 f.). The fact is that there is no conflict in such a case. The evidence of conflict would be hesitation and fluctuation. Both the intensitive and the temporal dichotic differentials are conjointly effective in determining localization. Every binaurally determined localization is the resultant of these two causes. It is not a conflict of cues when the trajectory of a bullet is curved: it is a resultant of concurrent causes. And that is what happens in localization and perception in general.

It is not easy to decide what other specific criticisms should be advanced. Every careful reader of so large a book will have his list of 'errors,' many of which will turn out to be mere antipathies. Let me select a few from the field of sensation where I am most at home, and let the reader judge for himself how heinous are these departures of Woodworth's from my straight and narrow path.

First come two matters of definition. In spite of his desire to avoid systematic

issues, Woodworth has attempted to state a distinction between *sensation* and *perception* (pp. 450 f.): "*Sensation* points to the sense organs with their nerves and nerve centers as the object of study; *perception* points to the objects of the world which we know through the senses." The conclusion that I draw from this formula is that perception becomes sensation as soon as you find out about its physiology, that the visual depth which comes from light and shade is perceptual because light and shade are in the object, but that stereoscopic depth is sensation because retinal disparity pertains to the sense-organ. For feeling Woodworth avoids a definition, but with the result that he can not take positive ground on the question of mixed feelings (p. 241). If he means by *feelings* either experiential processes or reactions, then he could have found good experimental ground for a dogmatic denial of simultaneous mixture; but, if he means 'meanings,' why then, of course, feelings mix.

In the field of psychophysics Woodworth gives valuable consideration to two insufficiently understood matters: the dependence of the size of the DL upon the width of the category of equality, and the relation of the method single stimuli to the principle of central tendency (pp. 425 f., 446-449). I applaud his treatment, and then add two complaints. The DL is not "more directly obtained" by the method of just noticeable difference than by the method of constant stimuli (p. 424), for the data from both methods have the same logical implications (if serial contradictions are ignored), and the size of the DL in both methods depends on the attitude of *O*, as to whether he uses a wide or a narrow category of equality. My other point is that the principle of central tendency, having been introduced so successfully with the method of single stimuli, ought also to be applied to the exposition of the relativistic nature of color constancy (pp. 599-801, 614, 616), for most of the magic disappears from the constancy-phenomenon when it is considered in this way.

The discussion of tone has elicited dissent from me a little oftener than has any other topic. Here are my complaints. Pitch difference and musical interval ought not to be equated (p. 504 f.), for they are different and the non-linear relation between them can be determined (cf. Stevens and Davis, 76-84). The exact ratio as the determiner of musical interval has gone, surely, into history, yet Woodworth says: "In music a slightly mistuned interval is not almost as pleasant as the true interval but quite unpleasant. In music the exact ratio counts heavily" (p. 386). Does it? when the musical *O*s who like the third best among the intervals still like it best in equal temperament where it is 16 cents off? when they like it best with a vibrato that varies it through 30 cents? The exact ratio does not count heavily for tonal fusion (Brues, 1927), and there is unpublished evidence (Newman) that it does not for pleasantness. Then Woodworth says: "According to Köhler's findings simple tones do have vowel quality, and vocality should be added to the list of tonal dimensions" (p. 514); but that, I think, is a non sequitur, for Külpe's rule that it takes independent variability to establish separate attributes still holds. Perhaps vocality *is* simply the pitch. Finally I have to say that beats do not establish difference tones (p. 515) unless there is distortion of the wave-form. When there is no distortion, Ohm's law works and the components are heard out without a difference tone, but distortion so changes the wave-form that a Fourier analysis or Ohm's law produces the beat frequency as a component. Perhaps sensation forms too technical a field for perfect adequacy in the general encyclopedist.

My chief regret about the chapter on color vision is that Woodworth has neglected the cortical theory of color vision. He mentions it for after-image (pp. 560 f.) and

for binocular mixture (pp. 574 f.); and he mentions Helmholtz's *unbewusster Schluss* for contrast (pp. 569 f.), which is not the cortical theory I have in mind. Yet he needs such a theory for his discussion of color mixture and color blindness (pp. 555 f.). One of the best arguments for the cortical theory is that dichromats, who can match all hues with mixtures of two, nevertheless see yellows and blues. The thought is that the retina is trichromatic and that the brain is tetrachromatic, with the paired incompatibilities of the Hering theory (no reddish greens) holding, not at the retina, but in terms of central mutual inhibitions.

Of the skin Woodworth says conventionally: "While not conclusive, the evidence is rather in favor of localized receptors and punctate sensitivity" (p. 460). To that I reply: Of course, there must be localized receptors in the skin, just as there are in the retina and the organ of Corti. Anything else would be a neurological marvel. And, if the receptors are localized, sensitivity will have to be punctate if only you can circumscribe the effect of the stimulus enough, and not otherwise. Obviously strong stimulation could not be punctate, for pressure spreads and tissues conduct heat. Thus I think Woodworth takes the classical problem too seriously.

And that—as nearly as one can paint a miniature of an elephant—is Woodworth's *Experimental Psychology*, which we have all waited for so long. It will have a great influence, and its encyclopedist-author merits the gratitude of professional psychologists.

Harvard University EDWIN G. BORING

The Intelligent Individual and Society. By P. W. BRIDGMAN. New York, Macmillan Co., 1938. Pp. vii, 302.

This may be briefly characterized as the reaction of a distinguished physicist to the problems of the universe within the fields of philosophy, psychology, sociology, and political science. As is well known, Bridgman, in his *Logic of Modern Physics* (1928), took the position that concepts in physics and other experimental sciences have no validity beyond the observations and that the observations can be regarded as true only for the particular conditions under which the observations are made. This he called the operational interpretation of concepts and if applied universally would result in a rigid empiricism. It would closely limit the inferences that could be drawn from any observation, if it would permit inferences at all. This work extends the empiricism of the former book to make a destructive analysis of the generally accepted axioms of most of the sciences and then in the later chapters makes a mild retraction of the resulting complete scepticism.

The book begins with a suggestion from physics that even the most fundamental concepts of science are not unambiguous. The measurement of velocity depends upon the position of the observer. Time and space are merely convenient notions and have little claim to absolute existence. Of the space-time dimensions of modern physics, Bridgman says, "there has been a great deal of bunkum about their situation." Cause is said also not to stand critical analysis.

Meaning is considered at length to the conclusions that words never have completely consistent meaning to all people. One can seldom find an actual event or object that corresponds to a word or phrase. Nevertheless the thinking of the ordinary man goes no further than combinations of words. Verbalization is one of the ultimate substitutes for thought on the part of even the most critical thinkers, scientific as well as philosophical and popular. The fundamental verbalisms usually receive support from mysticism. God, law, nature when assumed in explanations are

unknowable and while they give an emotional force to statements they add no real value to the conclusion. There is in the last analysis no complete justification for any statement that can be made except the operational one. That an idea works alone can be accepted. This test does not carry conviction as to the result of its application in any other instance or at any other time than the one in which it is found to work.

Social concepts are subjected to a similar destructive analysis. Ought and duty have the primary place in all social and ethical discussions. In usage they reduce to action in harmony with sublimated public opinion. Attempts to assign them to God's purposes does not stand analysis for no one knows God's purpose on any occasion. This is merely a verbalism to arouse emotions. Rights, property, morals of all sorts are verbalisms that can be given no rational justification. That they are of social origin is shown by the different values assigned to them in different societies, even in different civilized nations.

The state, law, justice are among the other social or political concepts that are reduced to mere verbalisms, that have grown up in society for the convenience of rulers or to justify social usages that at some time or another have made social coöperation or discipline easier. The sense of guilt is foolish as is any interest in the esteem one should have for one's self after one's death. Most of the fundamental values usually treated as the foundation of the social system are asserted to be the result of the extension of verbal combinations beyond the limit of applicability. They are the results of rationalization.

After all rational knowledge is questioned and all generalizations are rejected, Bridgman comes back to the emotional or instinctive life to justify decisions. The ultimate warrant for any action is the desire of the individual, just as the cause of any individual act is the mass of individual impulses. Even these can be generalized only on the unproven assumption that other members of society have the same emotions and impulses. One should expect to attain not absolute truth but a sense that a decision has been reached only after it has been scrutinized from as many points as possible. This attitude is favored by asking 'why' of each decision. The answer to the 'why' is not to be found in a logical formula or a rationalization but from an appreciation that the result will be pleasant to the individual, will fit into a broader scheme and will have desirable consequences even in remote applications. Ultimately the only justification is "I like it." To apply what the author would call a familiar verbalism "not reason but emotion or feeling must decide the course of action and in the last analysis truth and morality as well."

Bridgman's position has some resemblance to Kant's rule for ethical conduct so to act that the rule might be made a universal law. One "must not demand or allow for one's self as an individual any privileges which if assured for every one, would result in a kind of society that I cannot contemplate with satisfaction." Various present problems are tested by this rule. It is suggested that society might divide into those who like to use their brains and the great majority who do not. No ultimate way of convincing one group or the other can be expected. The man who enjoys thinking for its own sake will do it. The others will be content with verbalisms of ancient vintage. The latter will never accept the standards of the former. Bridgman also asserts that society must settle certain fundamental problems by physical force since not every problem can be solved by reason. He asserts that one must be ready to give over bitterness towards individuals who obtain their ends by force or guile. If they are successful, they must be accepted.

A judgment on the book is difficult. One cannot question the statements of the limitations of physical concepts or exact formulations in physics, nor can one question that there exists a general tendency to rationalize, to avoid the necessity for thought by verbalisms or by the use of mystic terms. One might apologize for laws and formulations as approximations and assert that with human limitations what they are, the formulations are necessary and not harmful, if they are not taken too seriously. Bridgman's objection to verbalisms may well be opposed to the Austrian logicians' assertion that words are the only mental reality in a point to point identity between external objects and thought. Bridgman may solve the problem by a denial of all reality.

Whether man will be satisfied to meet a scepticism more complete because more closely founded on fact than was the pre-socratic by the substitution for knowledge of personal feeling is a question. When subjected to Bridgman's criterion of rejecting all but concrete knowledge and making all acceptance dependent upon personal liking or dislike, the decision would lie with the personal reaction of each reader. Nothing more can be said.

University of Michigan W. B. PILLSBURY

Der Sinnbegriff als Kategorie der Geisteswissenschaften. By WILHELM KELLER, München, Ernst Reinhardt, 1937. Pp. 176.

This book, a prolegomenon to two forthcoming volumes, is an admirable, scholarly, and original study. Unfortunately, its ponderous, inflexible style, its interminably long and involved sentences, and its highly technical vocabulary—many of the terms being newly created—aggravate the task of the reader. To make himself understood, Dr. Keller explicates his thoughts by repeated definitions which have to be mastered before the points at issue can be comprehended. Exact for one slip (in one place he writes "A 'wooden iron' is an indeterminate being," and in another, "There are no indeterminate beings"), this book is a model of logical precision and integrative thinking.

The purpose of the book is "to take up the struggle for the logical and methodological bases of the *Geisteswissenschaften*" (p. 9), which are very different from those of the natural sciences as has been brought out by Hegel's classical antithesis between the philosophy of nature and of *Geist*. The *Geisteswissenschaften* aim at the comprehension of psychological and intellectual facts (real or imaginary, objective or hypothetical) and of ideally correlated situations. Psychology, sociology, and all branches of history belong within its scope. Philosophy, however, does not.

Within this book, Dr. Keller considers in separate chapters the following topics: "The primacy of method and its limitations;" "The relation of method and 'object' (*Gegenstand*);" "The 'object' of the *Geisteswissenschaften*;" "Meaning, understanding, and insight;" "A basic plan of a theory of the sciences."

The method of the *Geisteswissenschaften* has not only to determine its 'objects' but it is itself an intellectual phenomenon and, as such, it has to comply with the requirements that it sets up for its objects. In contrast to the natural sciences—which contradict themselves in applying themselves mechanistically to themselves—the *Geisteswissenschaften* are not only sciences of the mind but they are also sciences of themselves. They must accordingly, in their affirmations, justify their own existence. Thus psychology, for example, must continuously reëxamine and affirm its own

foundations, being equally a science of facts and principles. The author attempts to escape the infertile discussion of method alone by treating all the *Geisteswissenschaften* together as an entirety.

In placing the concept of meaning (*Sinn*) in the focal point, the author intends to illustrate its fundamental character. Indeed, the analysis of the categories of 'object' and 'method' is undertaken not only for methodological clarification but also, and principally, for the reason that it gives the first indication of what 'meaning' and 'meaningfulness' (*Sinnhaftigkeit*) really are.

The valid conditions of the *Geisteswissenschaften* are in general determined through cognition. Therein lies the reason for the affinity with philosophy. The *Geisteswissenschaften* become philosophical as soon as they treat of the fundamental characters of the intellectual structure. In contrast to the nativist, as well as to the empiricist, Dr. Keller points to the dynamic processuality of cognition in its anthropological and ontological aspects.

Meaning is a fundamental category, not only in the actuality of its application but in its inner importance It is not merely an internal affair of method, but it is initially constitutive of the objective being. It is the expression of a functional situation which holds together the 'being' and the 'being an object.'

Dr. Keller's thesis augurs well for the *Geisteswissenschaften*. In emphasizing the dynamic and progressive aspects of cognition, it deviates from traditional epistemology, and it challenges the scientific psychologies of today which are diversely ameliorated modifications of inherited British associationism.

The theories espoused are subject to criticism, but a critical review should await the forthcoming volumes in which the author promises to test and to substantiate his theories.

New York, N.Y. LUDWIG SCHOPP

A Biological Approach to the Problem of Abnormal Behavior. By MILTON HARRINGTON. Lancaster, Pa., Science Press Printing Co., 1938. Pp. 459.

In his *Wish-Hunting in the Unconscious* Dr. Harrington attempted to annihilate psychoanalysis; in the present book he attempts the praiseworthy task of providing "a new approach to the problem of abnormal behavior" which calls into account "the outstanding facts of anatomy, physiology, and psychology." In Part I (First Principles, 66 pages) he defines science as a system of conceptual constructs which provides prediction and accepts the requirements of a scientific theory as precision, simplicity, harmony with the facts, and comprehensiveness. All behavior is assumed to exist as the action of an anatomical mechanism which is governed by fixed laws. Conscious states are regarded as epiphenomena caused by the same bodily processes which cause behavior, and useful, therefore, as indicators of these processes.

In Part II (Psychophysiology, pp. 69-283) Dr. Harrington describes behavior as mediated by the nervous system which consists at once in a complicated reflex arc and a generator and reservoir of energy. Afferent impulses from stimulated sense organs (internal and external) produce active energy within the nervous system and a state of nervous tension. Psychological adjustment is finding relief from this state of tension and is achieved when the energy flowing out to the effectors has produced behavior which relieves the organism of the stimulus. This relief may be achieved in five ways, by "accomplishment," by "withdrawal from the stimulus,"

by "modification of postures," by "modification of thought," and by "disabling the machine." Four tension mechanisms (pain, pleasure, anger, laughter), the structure of the nervous system which is altered with every new adjustmental act (learning), and the *law of confluence* determine the form of action taken in achieving adjustment. The law of confluence states that when the organism is subjected to two or more stimuli at one and the same time, there is a tendency for the impulses aroused to coalesce. Justification for this law is found in the phenomena of conditioning, inhibition, and "reënforcement" (Sherringtonian summation), the same phenomena which the law is designed to explain (a beautiful example of circular logic). The James-Lange theory of emotion is adopted, and systematic but over-simplified conceptions of the physiological processes underlying sensation (*e.g.*, passage of an impulse originating in a single receptor to the brain), memory, thought, and anticipation are described.

In Part III (Psychopathology, pp. 287-431) psychopathology is defined as "the scientific study of why we misbehave and why we are unhappy and suffer pain." The requirements of the situation, the stimulus that calls forth the behavior, and the nature of the organism, determined in turn by heredity, education, and state of bodily health, are combined in causing behavior. Abnormalities are divided into two types: those of *impulsion* and those of *adjustment*. Every person suffers at some time from abnormal impulses, but they defend themselves by means of adjustment. Adjustment may fail, however, because relief from tension is obtained through unapproved accomplishments, withdrawals, or modifications of thought and posture (mal-adjustments), because relief from tension is not obtained at all (non-adjustment), or because the troubled person "disables the machine." A wide range of the phenomena of psychopathology are thus given systematic organization and explanation.

Except for an irritating interspersing of some form of "the answer is obvious," the style of the exposition is simple, clear, and should be interesting even to a lay reader. As Harrington's system serves well to classify and to account rationally for the phenomena of psychopathology, his book might serve well as an introductory text in mental hygiene, were it not for two defects: (1) His failure to take into account the facts uncovered in neuro-physiology, physiology, and psychology during the last 20 years would be obvious to students who had had courses in general biology and general psychology. (2) His hostility toward other contributors in the field of psychopathology, especially Freud, makes his book a bad example for inculcating an unemotional critical attitude in elementary students.

As a "new approach to the problem of abnormal behavior" which calls into account "the outstanding facts of anatomy, physiology, psychology" it falls far short of the mark. Psychopathology is in dire need of a major theoretical contribution which will marshal and integrate these facts, clarify the issues, and open new avenues of research. Harrington has not done this. He refers to the contributions of Sherrington and a few others when they fit into his system, but when the facts of research do not fit the reader hears nothing of them.

Harrington bitterly criticizes Freud for providing a metaphorical motivistic account of abnormal behavior, but his own constructs, "energy," "tension," "confluence," and his restatement and elaboration of McDougall's drainage theory are no more precise and have no more of an operational basis than have Freud's "libido," "cathexis," and "unconscious." Harrington has contributed an interesting rational

system of physiological psychology which organizes the phenomena of psychopathology, but is mechanistic metaphor better than motivistic metaphor?

Brown University J. McV. HUNT

Die Technik der analytischen Psychotherapie. By WILHELM STEKEL. Bern, Switzerland, Medizinischer Verlag Hans Huber, 1938. Pp. 317.

After thirty years of active psychotherapeutic practice, and in celebration of his seventieth birthday, Wilhelm Stekel, author of numerous books on special branches of abnormal psychology, sets forth for beginners the fruits of his experience. There are many paternal passages: "What I am here reporting is an extract from several decades of experience. I hope that it will be some assistance to my younger colleagues (p. 40). One learns . . . only by experience; beginners will do well to hold closely to the rule at first (p. 70). Patients can, under certain circumstances, bring about consequences which may prove very distressing for the beginner (p. 78). Many of the matters touched upon in this book are presented as a warning for those who, without a proper calling or sufficient skill, would attempt to occupy themselves with psychoanalysis" (p. 311).

It is within this framework that the book is written. There is but little speculation and much in the way of practical counsel; almost nothing of what one usually expects from a psychoanalyst: no unconscious, no libido, no id, neither is wish-fulfillment mentioned, nor are there instances of the Oedipus-complex; the whole concept of the castration-complex is criticized rather than supported. Instead there is an abundance of advice about how to begin the analytic inquiry, when and how to bring up the question of fees, what to do about members of the patient's family, when to encourage and how to discourage emotional transference to the analyst; finally, how to break off the analysis without upsetting the patient's newly found equilibrium.

Stekel warns his readers against Freud and the "orthodox" psychoanalysis. His criticism rests chiefly in the contention that Freud is essentially unscientific in having ordained fixed rules and methods which it were heresy for his followers to disobey (pp. 7 f. and 312 f.). Occasionally hypnosis is useful, but Freud has banned hypnosis; by no means is the method of free association always best, but Freudians are constrained to employ it in every instance regardless. But the most serious indictment brought by Stekel has to do with the time required for a psychoanalytic treatment: a matter of years in the majority of cases! Stekel maintains that a successful analysis can usually be completed in from two to four months. This is possible, he argues, *only* if one is prepared to substitute for the Freudian method of passivity his own method of active participation in guiding the patient's thought processes. In a word the analyst must possess the special abilities of a detective and of a diplomat. Patients *invariably* seek (even when they disclaim it the loudest) to conceal their real difficulties: to get at the truth one must proceed as a detective would do in ferreting out a criminal and establishing evidence necessary for a conviction. But mere analysis is not enough if one's purpose is to cure the patient. Analysis must be followed by a creative, intuitively guided process of *synthesis,* and it is here that the talents of a diplomat are essential.

It would be impossible in a short review to refer specifically to the many techniques outlined in this book. Almost never should the analyst undertake to treat a patient without an agreement on both sides that a week's trial must come first. Fees must be specified at the very beginning and patients should agree to make

weekly payments. Above all the analyst must discover at the earliest possible moment what form of *resistance* the patient is going to put up against a successful outcome of the treatments. Indeed, to this point the author returns again and again: if there is any exceptionless rule in psychotherapy it is, he believes, that one's patients are determined not to be cured. Usually the only way open to the analyst is to secure the emotional transference of his patient to an extent such that the latter will abandon his illness to please (or to spite!) the analyst. Thus transference is regarded by Stekel as of the highest possible importance, for it is only through its agency that resistance can be overcome, and it is only by overcoming resistance that positive results of any value can be achieved.

This is a good book. Clearly and vigorously written, it makes no pretense to being more than it is: a handbook of psychotherapeutic technique from the analytic point of view. Stekel admits that even 'orthodox' psychoanalysis has by now abandoned many fruitless notions; his own contribution is an argument for a revision of viewpoint such that psychoanalysis will disappear in favor of character analysis. This is not the principal theme of the present book, however. Most of its space is devoted to a straightforward and illuminating exposition of technique.

University of Arizona WILLIS D. ELLIS

The Brain and Its Environment. By JOSEPH BARCROFT. New Haven, Yale Univ. Press, 1938. Pp. vii, 117.

This book consists in three lectures given at Yale University on the Dwight Harrington Terry Foundation. The Foundation was established to further the "building of the truths of science and philosophy into the structure of a broadened and purified religion." In following out the details of this bequest, Sir Joseph Barcroft has summarized his own and other relevant studies on the chemical environment of the central nervous system and has amplified in this connection a true understanding of Claude Bernard's now famous phrase, "The constancy of the internal environment is the condition of the free life."

In the first lecture, Sir Joseph Barcroft describes the changes which take place during the fetal period of life in the chemistry of the blood of the developing organism. Here he emphasizes the preparatory nature of fetal development and brings much experimental evidence to bear against "a theory that is often paraded; namely, that the stimulus to growth is use." In connection with oxygen saturation in the blood which feeds the fetal brain, there is a drop in the fetal sheep from about 75% at 110 days to about 49% at 140 days. The former is an oxygen saturation of about that of an acclimatized person at an altitude of 20,000 feet; the latter saturation "brings you somewhere near the stratosphere." The author then well asks: "What can be the activity of the central nervous system under these circumstances?"

A consideration is further given of the type of behavior that can be elicited during various periods of fetal life. In general, the author suggests that the first receptor—central nervous system—effector actions are jerky movements. These then become more limited, and eventually inhibition develops, obscuring all such movements but allowing adaptive behavior. In the late fetus, however, if the chemical environment is changed by an even greater decrease in oxygen saturation, the fetus reverts progressively to earlier forms of fetal activity ending before death in the jerky movements which first characterized fetal response.

In the second lecture, the author points out that at birth many functions of the

brain spring into action which before that time have remained latent. He says that this change is due mainly to two things: (1) the change in the stimuli which may act upon the now free organism's sense organs; and (2) the more effective aeration of the blood. The author's own words are "that the foetus of the sheep even up to birth presents few if any signs of consciousness. Within a few minutes after birth, . . . the lamb lifts its head and tries to get up. It is plainly awake, and may be sent to sleep once more either by cold or by reducing the amount of oxygen in the blood. These matters must await further investigation; but it is evident that the higher faculties need a higher tension oxygen supply than those of a more vegetative nature."

The reviewer's experience with fetal mammals of species other than those worked with by Sir Joseph Barcroft does not lead him to the view that in general prenatal life can in any profitable way be considered as "unconscious" as contrasted with a "conscious" postnatal life. This difference of opinion may be due to differences in the use of the word "consciousness" as well as differences in the effective oxygenation of the blood in the types studied by the author and the reviewer.

Finally, in his last lecture, the author turns to a consideration of the mental efficiency correlated with such properties of the blood stream of the brain as reduction or rise in temperature, oxygen want or oxygen excess, carbonic acid deficiency and excess, and finally, blood sugar.

In conclusion, Sir Joseph Barcroft says: "We have therefore arrived at the point where we can say not only that the form of life—man—which has developed an intellectual power far beyond any other, is that in which the constitution of the fluids that bathe his cells is most exactly regulated; but also that the most immediate effect of interference with the chemical or physical properties of the blood is impairment of the higher qualities of the brain."

In the reviewer's opinion, this small book deserves real consideration by psychologists.

Tufts College LEONARD CARMICHAEL

The Psychology of Speech. By JON EISENSON. New York, F. S. Crofts & Co., 1938. Pp. xiv, 280.

The Psychology of Speech is divided into five sections: (1) "The Nature and Origin of Speech;" (2) "Basic Psychological Aspects of Speech;" (3) "The Development of Speech and Language in the Child;" (4) "Personality and Speech;" and (5) "The Psychology of the Audience." The first three sections constitute an integrated whole, since they are based upon common 'principles.' The other two are more or less distinct essays upon their respective topics.

In considering the various theories of the origin of speech, Eisenson inclines toward the 'oral gesture' theory of Paget and earlier writers. The 'ding-dong' theory is regarded as similar to the divine origin theory and as mysterious and absurd. While there is a great deal to recommend the oral gesture notion, this summary treatment of the 'ding-dong' theory is scarcely fair. It seems to the reviewer that modern approaches to language by way of 'phonetic symbolism,' and the 'physiognomic' character of speech sounds are closely related to the 'ding-dong' theory. They have the additional advantage of suggesting *experimental* attacks upon speech. Yet these notions are not mentioned at all by Eisenson, and his only reference is Mark Twain.

In the second section, the relations of speech and emotion, the application of

principles of learning to the development of speech, and the psychology of meaning and thought are discussed. In addition, there is a preliminary chapter upon the nervous system which seems out of place since so little of it has to do with mechanisms of speech.

The principles of learning which are believed to cover the development of meaning in words and the development of concepts in general are the familiar 'law of effect,' associative shifting (conditioned response), and associative spread. These, set in a context of stimulus-and-response, explain everything. When we add that the definition of thinking is the familiar 'talking to one's self,' the reader will note the familiar ring which all of this has. There are many objections which may be raised against the simplified picture of learning implied by these principles, and there are many ways of showing the inadequacy of such a superficial definition of thinking, but to present them here would exceed the limits of a review. We can only suggest that Eisenson's book would have been more useful if he had not accepted his 'principles' so uncritically.

The succeeding section on the development of speech in the child constitutes an excellent and brief summary of our present knowledge on the descriptive side of speech development. It is only when Eisenson attempts to *explain* the genesis on speech in terms of his principles of learning that the section becomes unsatisfactory. Again the explanation seems superficial and oversimplified.

We pointed out above that the last two sections are quite distinct from the first three and from one another. The disorders of speech are considered from the point of view of personality and its disorders rather than in terms of the principles of the earlier sections. Speech is here regarded as an expression of the total personality, and speech disorders as indicators of maladjustment of the personality. As a result of this logic, Eisenson considers not only the usual aphasic disorders and stammering, but also speech characteristics of the blind and of organisms suffering from more general psychological disorders such as schizophrenia and manic-depressive psychoses.

The final section on the "Psychology of the Audience" is a résumé of the well known facts and theories of the psychology of groups and audiences, and of the factors in gaining and holding the attention of listeners. The only difficulty which we may find here is that no experimental evidence concerning the *specific* application of the general facts to the speech situation is presented. For example, it is simple enough to say that change is one of the factors which is likely to secure the attention of an organism, but this does not tell us just what sorts of change or how much change is useful in addressing an audience. Eisenson does not see that these latter questions can be answered only by experimentation. Instead, he contents himself with casual observation and logical deduction from the general facts.

If called upon for an 'absolute judgment' of this book we should say that it is neither an adequate descriptive or 'theoretical' account of speech, nor a useful or practical handbook. It does, however, represent adequately the status of the field at the present time, although it contributes little that is new.

Cornell University T. A. RYAN

A Source Book of Gestalt Psychology. By W. D. ELLIS, with an introduction by Kurt Koffka. New York, Harcourt, Brace & Co., 1938, Pp. xiv, 413.

In preparing this source book Professor Ellis has answered a need which has been felt keenly by many teachers of experimental and systematic psychology, and

in doing so he has set a standard of excellence which merits unqualified praise. For altogether too long the German language and the relative inaccessibility of many German publications have virtually excluded from consideration by American psychologists the results of some of the most important German research. The *Psychologische Forschung*, for instance, has a lamentably small circulation in this country; Bethe's *Handbuch der normalen und pathologischen Physiologie* is prohibitively expensive; and such a journal as *Melos*, in which Von Hornbostel's classic paper on the unity of the senses appeared, seldom finds its way into a psychological library. Yet publications such as these contain some of the finest fruits of German psychological research. Although the current abstracts of German articles are useful as guides to reading, they cannot because of their brevity serve as substitutes for the original material; and writers of textbooks, on the other hand, are bound to select and evaluate as they report. Professor Ellis has attempted to steer a middle course between abstracting and interpreting. The book contains digests of 35 significant contributions to German psychology, each reduced without change in topical order and without editorial interpretation to approximately one tenth of its original length. A system of marginal page references indicates the exact amount of condensation, and enables the reader to find without difficulty the corresponding discussion in the original text. In keeping with the experimental character of the selections, the figures and tables have for the most part been retained, and extended historical and theoretical discussions have been either greatly reduced in length or eliminated altogether.

As the title of the book indicates, Professor Ellis has drawn his selections from the literature of Gestalt psychology. The wisdom of such a plan is obvious, for it has served at the same time to give coherence to the whole work and to present Gestalt psychology as a developing experimental movement. To those who still harbor the delusion that Gestalt psychology is primarily speculative, the book should bring a flood of illumination. Of the 35 contributions summarized, 27 deal with special problems of perception, motivation and thinking, in which actual experiments are reported, and the remaining 8 include summaries of Köhler's book on physical gestalten and Wertheimer's lecture before the Kant Society in Berlin.

It is inevitable that some readers will disagree with the selection of articles and with the treatment of those which have been selected. It is regrettable, for instance, that nothing published after 1929 could be included, in consequence of which there is no discussion of the recent work on memory-traces; that Köhler's important article, "Zum Problem der Regulation," had to be omitted; that more of Lewin's experiments could not have been reported. To make such demands, however, would be to ask for a book of unwieldy size, and even then all readers would not be satisfied. The book should be evaluated on the basis of its contents rather than of its omissions. The reviewer is satisfied that every contribution included is of lasting significance. He has, furthermore, compared several of the summaries in detail with the German originals, and has been deeply impressed by the skill with which Professor Ellis has caught and reproduced the spirit of the original articles without permitting himself to slip into the cumbersome style of a literal translator. The book should prove of great value both to teacher and to student. It is to be hoped, too, that it will stimulate other scholars to undertake similar tasks in other fields of psychology.

Swarthmore College R. B. MacLeod

Psychology and Life. By FLOYD L. RUCH. Chicago, Scott, Foresman & Co., 1937. Pp. xiv, 679.

Working with Psychology; A guidebook for use with *Psychology and Life* and other introductory texts. By FLOYD L. RUCH and NEIL WARREN. Chicago, Scott, Foresman & Co., 1938. Pp. ix, 214.

The number of recent texts in elementary psychology suggests, among other things, that there is rather general discontent with the first course in psychology. *Psychology and Life* attempts to remove these sources of discontent. Early in the book personality and intelligence are rather fully treated, and then the author gradually leads up to the more traditional subject-matter.

It is the reviewer's experience that this sequence of topics serves to arouse the student's interest in the chapters on sensory phenomena, attending, perceiving, learning and thinking. This heightened interest, not commonly noticed when other texts following a more traditional pattern are used, may be attributed to the fact that the student comes to appreciate the ephemeral state of our knowledge concerning personality. Consequently he accepts, and even desires, more substantial subject-matter.

Ruch's book is almost completely psychological in content. It contains no chapter on the nervous system, although it does refer to neural and humoral mechanisms when such references are pertinent. The most significant virtue of this book is its predominating emphasis upon information which can function in the lives of those who study it. The following citations from the subject index are typical: automobile accidents, Communism, 'crushes,' friendship, homosexuality, strikes, and marriage. Important psychological principles emerge from homely example and laboratory experiment. The book, however, makes no concessions to scientific rigor and caution.

Some will question how well Ruch has executed his announced program. Although the reviewer believes he has succeeded reasonably well, he will be disappointed if future textbook writers building upon the foundation laid down by Ruch do not erect a more finished structure. Many improvements might be suggested. The reviewer is especially disturbed by the choice of pictures which introduce the several sections. Many of them are inconsequential, adding little to the value of the book. The chapter on individual differences should be omitted since this topic would have more meaning for the student if it emerged from a discussion of personality and intelligence. The chapter on the origin of individual differences might better follow that on intelligence and discuss the heredity-environment issue alone, leaving maturation for consideration with learning.

The Guidebook is a valuable pedagogical device. The group exercises are especially well worked out and most of them can be performed outside of class. The self-tests have the defect of all self-tests in that they encourage parrot learning.

Western Reserve University CALVIN S. HALL

The Pituitary Gland. Edited by WALTER TIMME, ANGUS M. FRANTZ, and CLARENCE C. HARE. Vol. 17 of a Series of Publications by the Association for Research in Nervous and Mental Disease. Baltimore, Williams & Wilkins Co., 1938. Pp. xxiv, 764.

This handbook summarizes and evaluates the present status of research on the pituitary gland. The first section, dealing with the anatomy of the gland, has contributions by Tilner; Wislocki; Severinghaus; Rasmussen; and Rioch. The second

section, dealing with physiology, has contributions by Evans; Lee; Mortimer; Selye; Hisaw, Fevold and Greep; Thomson; Collip; Long; Riddle and Bates; Engle; Kurzrok; Kurzrok and Smith; Flohman; Bissonnette; Atwell; Richter; Ranson, Fisher and Ingram; Gersh; Geiling and Robbins; Lewis; Sittenfield; and Perla. The third section, entitled *General Considerations,* has chapter by Timme; Rubinstein; Goldzieher; Lurie; Richter and Eckert; Pardee; Herrick; Stockard; Beck; Hare; Horrax; Shelton; McKinney and Pardee; and Putnam and Davidoff.

The psychologist will be well rewarded by reading this whole book, but naturally he will be most interested professionally in certain chapters. Among these is Engle's treatment of the relation of the anterior pituitary gland to the physiological problems of puberty with special reference to the development of the male and female genital system at this period. The author emphasizes the importance of allowing the adolescent organism to achieve homeostatic stability by itself without the expression of anxiety or the use of premature therapy. Kurzrok's and Smith's treatment of the menopause will also be of interest to those who are concerned with the personality changes which occur at this time of life.

The chapter by Bissonnette on the influence of light upon pituitary activity is of direct concern to all students of behavior. This investigator here reviews in brief form the experimental work which shows that in certain animals, the sexual cycle is, in part at least, a function of the time and character of light stimuli present in the environment of the animal tested. Daily period, intensity, and even the wave length of the stimulating light are important. The eyes are the normal receptors for this stimulus, but in the duck and possibly some other animals, the pituitary gland itself may be stimulated directly by light falling upon it, thus directly modifying the sex cycle.

Richter's study of the pituitary gland in relation to water exchange is important in its own right and because of the ingenious experimental procedure used with his trial rats. Richter and Eckert also report that hypophysectomized rats are very inactive, show a reduced interest in food and water, and no interest in sex activity but that they build very large nests.

Rubinstein's interesting chapter on brain and body weight leads to the conclusion that the differences in brain weight between the sexes is independent of the influence of sex hormones.

Reading this book gives the psychologist many suggestions concerning some of the most complex and obscure factors which underlie human personality differences. The comparative psychologist will also be struck with the large amount of significant controlled behavior study which must sometime be undertaken in this field, especially in connection with the mechanisms of behavior activity which are rhythmic.

Tufts College LEONARD CARMICHAEL

Introductory Child Psychology. By WILLIAM A. KELLY and MARGARET REUTHER KELLY. Under the general editorship of Joseph Husslein, S.J., Milwaukee, The Bruce Publishing Co., 1938. Pp. xi, 413.

This book, according to the authors' foreword, is intended primarily for use in Catholic colleges and teacher-training institutions, as well as for Catholic parents. Thus we observe that "Catholic Philosophy has been appealed to throughout the volume in order to interpret the findings of modern psychology in the light of Catholic faith and tradition. The substantial foundation of the book is the principle that the child is a compound of body and soul, endowed with a free will, responsible

for his actions, destined for eternal life. The spiritual, volitional, and moral training of the child has been emphasized thoroughly as the most important part of the parents' and teachers' task in guiding and directing the child."

The book is divided into four parts: Part I, "The Bases of Growth and Development," considers such topics as the nature and scope of child psychology, growth and development, the physical bases of growth and development, the factors of mental growth and development, the dynamic factors of growth and development, and the social bases of growth and development. Part II, "Periods of Growth and Development," offers a descriptive account of the different periods of child development: the period of infancy, the period of early childhood or the preschool age, the period of childhood proper, later childhood, the period of preadolescence or the gang age, and the period of adolescence. "The Exceptional Child" is treated in Part III, and the two chapters deal, respectively, with the physically handicapped and the mentally different and the socially maladjusted. The central viewpoint of the book comes to a focus especially in Part IV on "The Social and Moral Guidance of the Child." Three chapters here deal with mental hygiene of childhood and youth, training in purity, and the formation of moral character.

The descriptive portions of the book follow the lines of current psychological orthodoxy; the interpretative parts, Catholic orthodoxy. Psychology, in general, is considered as a branch of philosophy, which "treats of the soul, its nature, powers, and manifestations. The *soul* is the substantial form of the body, the principle of life and of thought. . . . The term *mind* is used to designate the vital principle as the subject of mental activity, the principle by which the human being knows, wills, and feels." The facultative conception of mind is implicit, if not explicit, throughout the book. For example, memory is regarded as a power of mind, capable of storing up representations of past experiences and reproducing them later. The discussion of the topic of instinct is uncritical, and gives no evidence of recent evaluations of this concept. Respect for law, for the rights of others, honest, duty, reverence for purity, truth, and right, are considered as instincts.

An appendix of thirty-one pages contains "The Children's Charter" and exercises and problems for special study. The exercises are well conceived and should serve as helpful aids to the student.

Franklin and Marshall College PAUL L. WHITELY

The Science of Human Behavior. By WALLACE T. WAIT. New York, Ronald Press Co., 1938. Pp. xv, 335.

This is another addition to the list of general textbooks. Designed for students at the freshman level, it incorporates many of the advantages which characterize texts of the sugar-coated variety. It is well-written, neatly primed with what might be termed "student interest," and employs many of the more fashionable educational devices such as chapter outlines and a really well designed series of "problems for further thought."

Unfortunately, however, the disadvantages are also present. The author, in what was probably an honest attempt to avoid indoctrination and to arrive at an "healthy eclecticism," has allowed his book to suffer from a lack of organization and internal consistency. Thus, after two chapters in which structuration is upheld as one of the major determinants of behavior and a definite attempt is made to convince the student of a psychophysical monism, the structure of the human organism, save

for a lengthy chapter on the mechanisms of heredity, is almost completely ignored. The other major determinant is identified as the effect of environment upon the inherent structure but stimulation is never discussed and learning is relegated to a chapter of some seventeen pages. The topic of conditioning is exhausted in two pages while an admittedly unsatisfactory account of the Gestalt interpretation claims five. The student must therefore find himself in a somewhat unfortunate position. He is told what to call the principles of explanation to be employed but is forced to remain quite ignorant of their nature.

This major defect in the theoretical background leads to a number of minor inconsistencies, misinterpretations, and major omissions. Important questions which might be expected to give the student or the instructor some difficulty are sacrified for the more sensational, easy ones. Where difficult problems are discussed they are often disposed of by the device of "explanations" which are more facile than useful, more popular than thoughtful.

In short, the sugar coat while pleasant is too thick. It is so thick, indeed, that one suspects the thinking student will be inclined to wonder why the word science appears in the title at all.

Tulane University S. RAINS WALLACE

The Problems of Education: A First Course for the Orientation of Prospective Teachers. By C. C. CRAWFORD, L. P. THORPE, and FAY ADAMS. Los Angeles, Southern California School Book Depository, 1938. Pp. 239.

This is not an ordinary textbook. Rather it is an extended outline for a course of study. It consists of 207 topics, each occupying a page of the book, for which Issues, Activities, and Bibliographies are given. Under Bibliography the student is told under what headings the topic is listed in *Education Index* and *Educational Abstracts*. Periodicals dealing with the topic are next listed. Finally selections from the reserve shelf, consisting of twenty-nine carefully selected references, and two or three important books which deal with the topic are cited. The issues for each problem are clearly and succinctly stated. The activities comprise such things as debates, visits to schools, reports, and collecting data which bear upon the particular topic under discussion. Pedagogically the scheme is excellent. It follows a pragmatic philosophy faithfully. The reviewer was puzzled, however, as he read it. How could prospective teachers cover the course in a year or even in six years? Each topic with its wealth of reference material and suggestions for activities on the part of the students, seemed to contain sufficient material for at least a semester. Any student who was a member of a class that faithfully carried out all the instructions would find himself on the completion of the course at the M.A. or Ph.D. level of knowledge. Take, for example, topic 117, Connectionist (Thorndike) Psychology. The student besides looking up the appropriate material in *Education Index* and *Educational Abstracts* is requested to consult the *American Journal of Psychology*, the *Journal of Educational Psychology*, and *Psychological Review*, and to read specified sections from Caswell, Frasier, Myers, Strayer, Thomas, and Thorndike. In addition, he is expected to read or at least to consult Gates, *Psychology for Students of Education* and Thorndike's, *Fundamentals of Learning*. Now Thorndike's *Fundamentals of Learning* proves stiff reading for most graduate students, yet it is given as a reference for a first course of prospective teachers! Either a limited selection from these 207 topics must be made by the teacher, or the whole of them must be treated rather superficially.

As a mine of information for the teacher or the advanced student, the book is excellent, but in the opinion of the reviewer it will prove too difficult for most undergraduate groups unless the teacher is superbly good at his job.

For the psychologist the interesting feature of the book is the emphasis placed upon organismic and Gestalt psychology. For the teacher the stress upon the progressive point of view will prove to be the most interesting. Finally, it should be stated that the production is both workmanlike and consistent. It will probably be more useful for teachers and advanced students than for the group for which it has been specifically designed.

University of Toronto PETER SANDIFORD

Emotion and the Educative Process. By DANIEL A. PRESCOTT. Washington, D.C., American Council on Education, 1938. Pp. xviii, 323.

This is a report to the American Council on Education by its Committee on the Relation of Emotion to the Educative Process. It is a survey of research bearing directly and indirectly on the part played in education by all affective experiences, including feelings, emotions, and all attitudes having affective and emotional components. Prescott, chairman of the committee, wrote this report and selected the materials to be included supplied by seven members of the committee. The report is the outcome of four years of work, consisting for the most part of surveys of relevant literature and research, supplemented by Dr. Prescott's personal tour of various universities, clinics, and research centers. Plans for further research are to be made for areas of research which the present report shows to be undeveloped.

The findings of the survey are given in 10 successive chapters (II to XI), followed by a chapter of general conclusions. The chapter titles may serve as brief indications of the materials covered: II, Basic Affective Phenomena; III, Physiological Basis of Affective Experience and Behavior; IV, Patterning and Trainability of Affective Behavior; V, Affective Maturity; VI, Basic Personality Needs and Conditions which Frustrate Them; VII, Affective Behavior and Contemporary Social Institutions and Processes; VIII, The Influence of Affective Factors Upon Learning; IX, Affect and Education; X, Aspects of Education Needing Study; XI, Personnel Problems in Education.

Each chapter gives in survey fashion the basic theories, experimental methods and results of the references covered. Although these surveys are necessarily incomplete, a full citation of sources makes it possible for the reader to seek further information. The final summary chapter consists of a series of sections each one serving as a critical estimate of the present status of some special area of research, with an indication of the lines which further research should follow. The author has a clear realization of the complexity of the problems encountered, and emphasizes the need of pooling the knowledge and skills of a large group of experts in various fields of science and practice. Indeed the practical bearings of emotion on the educative process must wait for years of extensive research into fundamentals.

Wells College C. O. WEBER

An Introduction to the Fields of Psychology. By EMILY S. DEXTER and KATHARINE T. OMWAKE. New York, Prentice-Hall, 1938. Pp. 236.

The present volume is intended as a second book for elementary students. To introduce them into the various types of psychology, rather than the whole field of

psychology in general, it should be used in an introductory course. The volume is simply written and easily read and it might well serve to be a text for elementary psychology provided a few introductory lectures are given. The chapter headings, with the exception of those dealing with medicine, law, efficiency, and advertising cover topics which easily constitute single courses in a good department of psychology; namely, abnormal psychology, social psychology, child, animal, measurement in psychology, and experiment in psychology. Since each of these chapters is seldom more than fifteen pages long, it is easy to conceive that the subject can scarcely be covered in much detail, in fact, so little of the vast amount of material that can be covered in individual courses dealing with these subjects is presented that it is somewhat questionable whether each chapter by itself would be stimulating to the student who might wish to go on into one of these more specialized courses. The information, as given, is conventional. There is little that is controversial but also little that is highly modern; for instance, in abnormal psychology a very brief list of disorders is given, each one being amplified by only a short paragraph. The idea of dynamisms should not be touched upon in such a brief space but is. Similar criticisms might be made of each individual chapter but if this book is to be used in a course, which appears early enough in the college student's curriculum, it should be with amplification by the instructor to provide a means of revelation to the student that there is more to psychology than one finds in the usual beginners' course. It is systematic, simple, and satisfactory so far as it goes.

Recorder's Court, Detroit, Michigan LOWELL S. SELLING

Shamanism in Western North America: A Study in Cultural Relationships. By W. Z. PARK, Evanston and Chicago, Northwestern University, 1938. Pp. viii, 166.

The author's problem and conclusions will be of interest chiefly to specialists in American Indian ethnology. His goal is to ascertain interrelationships of tribal cultures reflected in the geographical distribution of elements in the shamanism of the Paviotso, or Northern Paiute of western Nevada. After describing Paviotso shamanism in considerable detail, he traces the distribution of similar beliefs and practices among neighboring tribes of the Great Basin, the Plateau, California, etc. Raw data of interest to some psychologists may be found in detailed accounts of dreams reported by some of his Indian informants. Shamanistic powers among the Paviotso are always received through dreams. Illnesses are also thought to be caused by dreams. In curing such cases, the shaman requires his patient to recall the content of the dream and the time it occurred.

West Springfield, Mass. MABEL F. MARTIN

A Simplified Guide to Statistics for Students of Psychology and Education. By G. M. SMITH. New York, Farrar & Rinehart, 1938. Pp. ix, 70.

Satisfactory interpretation of much of the material in psychology and educational psychology texts requires a working knowledge of elementary statistics. In the laboratory courses in these fields such knowledge is indispensable. The general practice is to introduce into the texts, either as separate chapters or as part of the discussion at appropriate places, a minimum of statistical explanations. Frequently the limitations of space prevent such material from being adequate.

This simplified guide, while not pretending to serve as a text in statistics, "is intended to clarify and supplement the work in general or laboratory courses." The

material covered includes a consideration of distributions, measures of central tendency and deviation, the use of norms and standard scores, reliability of the mean and of the differences between means, and correlation techniques and their uses.

At times there is an obvious tendency to talk down to the students. In a few instances the discussion is not complete enough to be clear to the elementary student. An example is the interpretation of correlation coefficients, especially the reliability of test scores. The book would be more complete if representative samples were described and tables for PE_Q and PE_r were included.

As a whole this simplified guide to statistics is excellent. The material is well chosen and clearly discussed. It will undoubtedly receive a hearty welcome from many instructors who have long felt a need for this sort of treatise.

University of Minnesota MILES A. TINKER

Progressive Relaxation. By EDMUND JACOBSON. 2nd. edition. Chicago, Univ. of Chicago Press, 1938. Pp. xvi, 494.

The first edition of this book was reviewed at length in this JOURNAL (42, 1930, 473). The new material in the second edition consists principally of three chapters. The first discusses the measurement, by means of string galvanometer and amplification, of minute potentials due to slight muscular contractions. These measurements were needed particularly to serve as an objective check upon the results of training in relaxation. The records lead the author to give up the traditional view "that healthy muscles, even in the resting state, are always in a state of slight sustained contraction, commonly called 'tonus'." They also show clearly that relaxation can be cultivated by training. In the second new chapter is presented the author's well known work on the connection between imaginal and other mental processes and the activity of specific muscle groups. The third new chapter, mainly of clinical interest, is on the cultivation of quick relaxation and of slow progressive relaxation. Dr. Jacobson's contributions are of both theoretical and practical importance; they have helped solve certain problems on the relation between mental and muscular activity, and in addition they appear to offer therapeutic aid in the treatment of certain disorders involving "neuromuscular hypertension," which include spasticity of parts of the alimentary tract, mucous colitis, certain psychoneurotic conditions, and other troubles.

The Rice Institute FRANK A. PATTIE, JR.

The Development of Linguistic Skill in Twins, Singletons, and Only Children from Five to Ten Years. By EDITH A. DAVIS. Minneapolis, Univ. Minnesota Press, 1937. Pp. ix, 165.

Like Piaget, Mrs. Davis has emphasized the symbolic content, rather than the mechanical aspects of speech development. In addition, however, she has contributed a more objective technique, a more controlled selection of subjects, and a statistical analysis of her protocol data. Her subjects, aged 5½ to 9½ yr., included groups from six socio-economic levels, carefully equated in sex, intelligence, birth order, etc. In a "free" play situation involving specific materials, each child was told to explain or describe to the experimenter what was going on in the game, and fifty remarks were analyzed as to articulation, vocabulary, sentence structure, function (*i.e.* monologue, conversation, questions, answers, etc.; naming, comparing, describ-

ing, etc.). Combining all phases, Mrs. Davis found that only children were consistently superior; singletons and siblings were next in order; and twins consistently last in language development. No relation to bilingual environment was discovered. Girls were superior to boys, especially at the earlier age-levels, but the greatest differences coincided with differences in socio-economic status.

In both structural and functional aspects her data confirm the 'rule' that normal speech development is toward better articulation, longer and more complex sentences, larger vocabulary, and precision of symbolic content.

University of Michigan MARTHA GUERNSEY COLBY

Philosophie und Pädagogik Paul Häberlin's in ihren Wandlungen. By PETER KAMM. München, Reinhardt, 1938. Pp. 479.

Häberlin is a contemporary German philosopher. As such he regards himself although he is a Swiss citizen, born in 1878 in Kanton Thurgau, and at present professor in the University of Basel. Kamm, a student of Häberlin, has undertaken to describe the development in four stages of his teacher's philosophical views. He does this to a large extent by quoting from the published works (94 in number) among them two dozen books, some published in revised editions years after their first publication. From the author's representation, Häberlin appears as a clear and modern thinker, devoted to all branches of philosophy, religion, ethics, esthetics, metaphysics, and pedagogical speculation. Those interested in acquainting themselves with the several aspects of his philosophy will be greatly aided by Kamm's elaborate index of nearly twenty pages in finding within the book the commentaries to the quotations and the references to Häberlin's works, which constitute the author's labor.

Coral Gables, Florida MAX F. MEYER

Fundamentals of Educational Psychology. By W. B. DOUGLAS and B. F. HOLLAND. New York, Macmillan Company, 1938. Pp. xv, 598.

This is but one of the many introductory texts on educational psychology that have appeared during the last five years. It is very elementary and therefore might prove of interest to the teacher having a class with a limited background in psychology.

The text is divided as follows: Part I, a brief introduction of definitions and a discussion of scientific method; Part II, Environment and Heredity (taking up about a sixth of the text); Part III, on learning makes up about half the text, thus appealing to the instructor wishing to place a large portion of the class time in the field of learning; and Part IV, on Measurements in Educational Psychology takes up a generous fourth of the book.

University of Pittsburgh W. T. ROOT

The Philosophy of the Act. By GEORGE HERBERT MEAD. Chicago, Univ. of Chicago Press, 1938. Pp. lxxxiv, 696.

This is the third and last volume embodying the unpublished writings of a great American teacher and philosopher. The previous volumes were *Mind, Self and Society* and *Movements of Thought in the Nineteenth Century.* The present volume contains only fragmentary notes, mainly on philosophical matters, and adds little to the elucidation of Mead's position which was developed in a more systematic fashion

in the two other posthumous volumes. An excellent introduction by the editors gives a detailed and definitive analysis of Mead's point of view and his contributions.

Columbia University — THEODORE ABEL

The Real Use of the Unconscious. By CHANDLER BENNITT. New York, Dial Press, 1937. Pp. vii, 380.

This potpourri of Psychoanalysis, Theosophy, Yoga and Christian Science has for its themes: (1) that the conscious mind is factual; (2) that the unconscious is symbolic; and (3) that man's ultimate salvation lies in a fusion of the factual and symbolic. The book is worthless from a scientific point of view except as an exhibit of irrational writing and conclusions that are *non sequitur.*

Occidental College — GILBERT BRIGHOUSE

The Psychologist at Work: An introduction to Experimental Psychology. By M. R. HARROWER. Preface by K. Koffka. New York and London, Harper & Brothers, 1937. Pp. xi, 184.

This book was written with the avowed purpose of giving the layman some appreciation of what psychologists do—their problems, methods of attack, and modes of solution. It was evidently not written to entertain the reader, or to popularize the subject in the usual sense of the term. Rather it is addressed to the serious minded and intelligent layman who wishes to obtain some knowledge of what academic psychology is about. Naturally a short book best serves this purpose, and necessarily the number of topics selected for discussion is limited. The author recognizes that the selection of topics and their mode of treatment have been largely dictated by her predilection for the Gestalt point of view. Evaluated from this purpose and point of view, the book can be commended for its selection of topics, and especially for the directness, simplicity, and clarity of its exposition. Naturally the book is of no great interest to the professional psychologist, with the exception that it might well be used as supplementary reading in introductory courses.

University of Chicago — HARVEY A. CARR

Statistics in Education and Psychology. By ELMER R. ENLOW. New York, Prentice-Hall, 1937. Pp. ix, 180.

In this compact little volume the author has essayed the difficult task of combining a handbook and a textbook. He has succeeded rather better in his selection of useful formulas and tables than he has in writing a teachable text for the novice. The usual fundamentals, the frequency distribution, measures of central tendency and dispersion, percentiles, correlation techniques (first order, partial, and multiple), and errors of sampling and measurement, are systematically treated, wherever possible, under the headings: Definition, Interpretation and Use, Formulas, Derivation of Formulas, Mathematical Properties, and Methods of Calculation.

The interpretations are not always helpful to the beginner; the derivations are frequently casual and the mathematical properties are often gratuitous. The style is not facile, and compactness has often been achieved at the expense of lucidity.

The appendix contains a selected bibliography, a glossary of symbols, and seven mathematical tables: tables of squares, square roots, reciprocals, and common logarithms for numbers from 1 to 1000; alienation coefficients and values of $1 - r^2$; and various combinations of areas, ordinates, and abscissas under the normal curve.

The tables are of distinct value to the statistical field worker, but are not adequately explained for the uninitiated or linked to the text.

College of the City of New York — G. MILTON SMITH

Knowledge and Society: A Philosophical Approach to Modern Civilization. By the University of California Associates. Pp. ix, 417. New York, Appleton-Century Company, 1938.

The title of this work might have been somewhat more fittingly "Philosophy and Society." It is not an analysis of the social order, still less is it one of those studies to which the Germans give the name of *Wissenssoziologie.* It is an introduction to philosophy, an introduction in the proper sense—not a history of philosophy or a review of philosophical systems, but an exposition of the philosophical approach, bringing out its bearing on modern social and political issues. As such, it is lucidly and coherently written, remarkably unified for a work of composite authorship. It is the product of seven members of the Department of Philosophy of the University of California. It fulfills admirably its purpose as stated by the general editor of the Century Philosophy Series, as "an aid to any teacher whose major concern is to cultivate in his students the habit and enjoyment of reflection." But a book of this character might well serve another function. It can certainly be recommended to those sociologists and psychologists who without any real philosophical training commit themselves to philosophical positions, often without being aware that they are doing it.

Columbia University — ROBERT M. MACIVER

The Handicap of Deafness. By IRENE R. EWING and ALEX. W. G. EWING. New York, Longmans, Green and Company, 1938. Pp. xi, 327.

This book deals with the scientific study of the social and educational effects of deafness; the measurement and assessment of any remaining power to hear, and the development, through training and skill, of its use; the value of lip-reading and methods of learning to lip-read; the efficiency, characteristics and use of hearing-aids of various types and their suitability for patients suffering from different forms and degrees of deafness; the principles which underlie the education of totally, severely and partially-deaf children.

The book is scientifically sound, pedagogically wise, well illustrated, and socially helpful. It is by all odds the best book now available for its purpose. And it comes at the psychological moment when the invention of audiometers and introduction of hearing aids has done for the ear what the fitting of glasses did for the eye ages ago. This book should be in the hands of every superintendent and principal of schools and parents who are concerned about the hearing of their children. It should be sold at the news stands because it contains news and aids for young and old who suffer from loss of hearing. I have only one criticism, and that is of the price and would suggest to the publishers that if they cut the price in two they would make an excellent investment.

University of Iowa — C. E. SEASHORE

Le pithécanthrope était-il un pygmée? By PAUL BUYSSENS. Brussels, Purnal, 1937. Pp. 48.

As indicated by the subtitle, "Considérations sur la généalogie humaine, in-

spirées par les recherches recentes sur l'ancienneté at les caractères de certains hommes fossiles," Buyssens presents, in this little essay, the arguments in support of the thesis that the Pygmy was the first man. To the author there are two distinct lines of human evolution. Pithecanthropus is distinct from the Pygmies who were descended from him and who in the long course of evolutionary development produced all other human forms. Anatomical variation and wide dispersion of Pygmy populations are, in part, evidence cited in support of the main thesis.

The author weighs the chronological and filial relations of the Pithecanthrope, Sinanthrope, Solo (Java) man and the Australian and Rhodesian prehumans and decides that, despite cranial similarities and geological classifications as to antiquity, human phylogenesis is more complex and less certain than has been admitted.

This essay is intended to further clarify and extend the author's views on the problem of the Pygmy treated in his book *Les trois races de l'Europe et du monde* (1936).

Ohio State University SAMUEL RENSHAW

An Introduction to Child Study. By RUTH STRANG. Revised edition. New York, Macmillan Co., 1938. Pp. xv, 681.

Since its appearance in 1930, this book, which the author states is an "applied psychology of childhood . . . (with) emphasis . . . on the service aspect rather than on research methods," has been thoroughly revised and greatly enlarged. Its organization has not, however, been changed. It contains twenty-two chapters which are divided into six sections, as follows: Section I, infancy (including hereditary and prenatal influences); II, early preschool; III, late preschool; IV, primary school; V, preadolescence; and VI, adolescence. Every section includes a survey of growth and development, a study of how children learn, a discussion of special problems with suggestions for dealing with them, and an account of the techniques for observing and studying the child's behavior.

Dr. Strang's book is particularly well suited for parents and students of education. Her treatment of growth and intelligence testing are excellent, as are also the many suggestions for dealing with 'problem behavior' such as lying and cheating.

An appendix gives height and weight norms and lists of play materials. Its indices, subject matter and author, are excellent.

Sarah Lawrence College LAWRENCE JOSEPH STONE

The Psychology of Human Conflict. By EDWIN H. GUTHRIE. New York, Harper & Brothers, 1938. Pp. ix, 408.

Of the many volumes which have been published upon 'human conflict,' Dr. Guthrie's is, in the opinion of the reviewer, the best. By taking account of the results of recent researches in this field, the author avoids many of the pitfalls into which the earlier authors have fallen.

Dr. Guthrie's account proceeds from a homeostatic definition of life as 'the preservation of identity through the continued maintenance of essential constant states.' After a discussion of motives, desires, and interests, the principle of conflict, which is based on inhibition, is applied to the functional disorders. A notable aspect of the treatment is the author's insistence upon the rôle of the body in maladjustment and disorder.

The outstanding defect of the book is the imbalance between the first twenty-one

chapters and the last seven. The tone of the book and the character of its contribution tails off in these closing chapters.

In addition to a clear and charming style of exposition, the author possesses a sound grasp of his subject matter. His verbal illustrations and analogies are well-chosen and his exposition is interesting throughout the book.

Cornell University ROBERT M. LINDNER

Psychology in Everyday Life. By WALTER C. VARNUM. New York, McGraw-Hill Book Co., 1938. Pp. x, 444.

This book is designed as a text for non-professional students of psychology. It is popularly written and is replete with illustrations many of which are not explained or referred to in the text (*e.g.*, p. 32). Varnum stresses the practical. Difficulties are glossed over and conflicting theories and opposing points of view are avoided.

The author attempts in this book to make psychology interesting and easy and he achieves some success in both goals but at the cost of a distorted and false view. The exposition depends largely upon secondary sources and the recommendations for further reading are chiefly to popular books.

It is unfortunate that such a text should be placed in the hands of elementary students.

University of Newark FREDERICK J. GAUDET

Physiological Optics. By VERNON W. GRANT. Chicago, The Professional Press, 1938. Pp. 240.

This is a summary from a popular point of view of some of the facts of vision and related fields. Two chapters are devoted to the problem of reaction and the anatomical and physiological substrate of responses and then one each to the visual reaction system, visual perception, visual perception of space, attention in vision, visual sensations and illusions of vision.

The material is abstracted from well-known current works. There is nothing about its content or method of presentation to make it noteworthy. The order is not satisfactory in many cases, and the statements are not always clear. It will probably interest the general reader but will find little place with university classes.

University of Michigan W. B. PILLSBURY

The Kelley Statistical Tables. By TRUMAN LEE KELLEY. New York, Macmillan Co., 1938. Pp. 136.

The author's apology for these very complete tables is that years of experience have convinced him of the universal usefulness of the normal distribution curve in psychology. Table I gives values of x (deviation score), z (ordinate), $(pq)^{\frac{1}{2}}$, $(1-p^2)^{\frac{1}{2}}$, and $(1-q^2)^{\frac{1}{2}}$, to eight decimal places corresponding to areas under the normal curve (p), for values of p varying from 0.5000 to 0.9999 in steps of 0.0001, and from 0.9999 to 0.9999 9999 9 in smaller steps. The total area is unity. The inclusion of the radical values yields constants useful in correlational problems; namely, $(1-r^2)^{\frac{1}{2}}$ and $(1-k^2)^{\frac{1}{2}}$, for various values of r.

Table II gives values for Pearson's coefficient P, for values of $\chi/n^{\frac{1}{2}}$ and χ^2/n, and for various degrees of freedom. Tables III, IV, and V give cubic, quintic, and septic interpolation coefficients, respectively, for values of p from 0.500 to 1.000. Table VI gives square roots of numbers from 1 to 1000, to six decimal places.

Table I comprises the bulk of the volume. In view of the insufficiency of all older tables of this type and the increasing call for the derivation of abscissa values from areal observations, statistical psychologists will welcome these new extensive tables. Apparently all possible precautions have been taken to insure accuracy in computing the tabular values.

University of Nebraska J. P. GUILFORD

Teachers and Behavior Problems. By E. K. WICKMAN, New York, The Commonwealth Fund, 1938. Pp. 40.

This is a digest and condensation of the well-known study of *Children's Behavior and Teacher's Attitudes* carried out by Wickman and published by the Commonwealth Fund ten years ago (1928).

The account of the methods of the original investigation and the detailed results are omitted. A condensation of the results and their implications for education comprise the essential portions of the pamphlet. It should be useful to teachers and educators who are not interested in the details of such studies but only their application to educational problems.

University of North Dakota C. W. TELFORD

A Preview to College and Life. By CARL E. SEASHORE. Iowa City, Iowa, University of Iowa Press, Studies on Aims and Progress of Research, No. 55, 1938. Pp. 78.

This monograph contains a series of open letters to undergraduate and graduate students setting forth personal questions referent to certain problems in educational orientation. The author does not attempt to offer exhaustive answers to the questions proposed; he limits his treatment to a discussion of some of the more significant factors. The student looking for inspirational advice may gain from the discussion, but it is of no direct interest to the psychologist.

University of Maryland LESTER P. GUEST

The Psychology of a Growing Personality. By F. M. GREGG. Lincoln, Nebraska, The Personality Press, 1938. Pp. xv, 489.

This book contains a mixture of uncritical eclecticism, strained physiologizing, sentimental pap, cheap moralizing, and practical common sense. It is intended for character educators and for the common man and woman.

The carelessness of the author in preparing the manuscript and proof reading is everywhere apparent. Psychology is spelled "physchology" in the very first sentence. The following names are misspelled: Gesell, Woolley, Kuo, Descartes, Thom, and Berkeley. R. S. Woodworth becomes S. R. Woodworth. Vineland appears repeatedly as Vinland. Although these are inconsequential errors, they indicate the general characteristics of the volume. Psychologists will not find it a useful book to have in their library.

Western Reserve University CALVIN S. HALL